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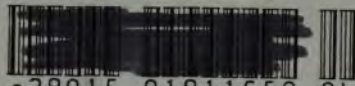
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BAND IV.

INTRODUCTION  
TO THE  
STUDY OF LANGUAGE:

A CRITICAL SURVEY OF THE HISTORY AND METHODS  
OF COMPARATIVE PHILOLOGY OF THE INDO-  
EUROPEAN LANGUAGES.

BY

B. DELBRÜCK.

AUTHORIZED TRANSLATION, WITH A PREFACE BY THE AUTHOR.



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## **INDOGERMANISCHER GRAMMATIKEN**

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A. LESKIEN, G. MEYER, E. SIEVERS, H. WEBER, W. D. WHITNEY,  
E. WINDISCH.**

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### **BAND IV.**

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A CRITICAL SURVEY OF THE HISTORY AND METHODS OF COMPARATIVE PHILOLOGY  
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## PREFACE.

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The character of the present work is mainly determined by the circumstance that it is intended by the author to facilitate the study of the "Grammars" which BREITKOPF & HÄRTEL are publishing, as well as the comprehension of comparative philology in its newest form.

The field of this "Introduction" is no broader than that of the above-mentioned "Grammars". Wherever in the following pages language, language-development, phonetic laws etc. are discussed, *Indo-European* language, *Indo-European* language-development etc., must alone be understood. I have felt the less inclination to discuss questions which lie outside the Indo-European domain, as for example those suggested by universal philology, because in reality the influence of philosophical linguistic research upon the science founded by BOPP has always been of slight account, and is very trifling at present. In limiting myself to the departments of phonetics and inflection I have also followed the "Grammars", but I must acknowledge that I should not, perhaps, have practised this self-denial if I had not just shown, in the fourth volume of my syntactical investigations (*Die Grundlagen der griechischen Syntax*, Halle, 1879), how in my opinion the comparative syntax of the Indo-European languages should be treated.

The book here laid before the public is divided into a historical and an analytical part. In the former the development of philology from BOPP's time to the present is roughly sketched,

with the especial aim of showing clearly how the problems which particularly occupy our attention today have developed naturally from what preceded them; in the second part the most important of these problems are discussed. In the fifth chapter (*Agglutination Theory*) I have expressed myself with more skepticism and reserve than formerly. But I have thought it best not to suppress my present views, since they are the result of mature deliberation, and I cannot believe that I shall ever have a more decided opinion on these points. In the sixth chapter (*Phonetic Laws*) I hope I have shown that the harmony between the representatives of different tendencies is greater than the uninitiated would be inclined to suppose, possibly greater than is recognized by many of these representatives themselves. All these discussions, as well as the historical part (chapters I—IV) and the seventh chapter (on the Separation of the Races), are not written for the narrow circle of my fellow-workers in this field, but mainly for those who do not make a special study of comparative philology. For this reason I have made use, so far as I was able, of the exceedingly abundant philological literature, but have quoted it only to a small extent.

In conclusion, I would like to say a word respecting my judgment of living scholars. In characterizing great men who are deceased, like BOPP and SCHLEICHER, I have imposed no restraint upon myself, but to proceed in the same way with reference to the living seemed to me unfitting. I have accordingly never attempted to picture in full the distinguishing characteristics and achievements of any living philologist, while I have not hesitated to define my position in regard to single views of my contemporaries. To this point I would fain call the attention of those readers who perhaps might be inclined to think that I have not done justice to this or that eminent living philologist.

Jena, August 20, 1880.

B. DELBRÜCK.

## AUTHOR'S PREFACE TO THE ENGLISH TRANSLATION.

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In writing the present work, my only thought was to introduce some of my countrymen to the study of the Indo-European languages, and to call their attention to certain points in the history and present condition of this study. Accordingly, the preface, as well as my treatment of the subject, were destined for German readers. On this account it seems to me especially important that the English translation should be preceded by a few words of explanation, or, I might almost say, of apology.

Above all, I would deprecate any comparison of this short and unpretentious "Introduction" with the extensive works of MAX MÜLLER, WHITNEY und SAYCE. I by no means aim to furnish an explanation of the nature of language in general, as these scholars have done; it is only my wish to show how a minute section of the whole linguistic mass has been studied, and in my opinion ought to be studied. It is true that in one chapter, that which treats of agglutination, I touch on more general ground, and it would perhaps have been better if here I had at least attempted to discuss my disagreement with SAYCE's views somewhat in detail. I hope that in case of a second edition I shall be able to extend my original plan in this direction.

Perhaps it may seem to the reader that not only the above-named English and American philologists have received insufficient consideration, but that the same is true of other foreigners, as for instance those exact scholars of whose views the "*Mémoires de la société de linguistique*" may be considered the exponent. I cannot wholly deny the justice of this criticism, but the following considerations will serve to explain my course. This book was written with the aim (how far it has been attained, I will leave others to judge) of contributing to the history of the German mind. It is universally acknowledged, by those who have traced the history of German development, that there is an immense gulf between the views of the Germans of today and those prevalent up to the fourth or fifth decennium of this century. This difference of view is almost as great in scientific fields as in the domain of politics. One side of this mighty revolution can be concisely expressed in the statement that we have passed from a philosophical epoch into a historical one. I attempted to show (as no one to my knowledge had done before) that the science founded by BOPP stands in evident connection with the philosophical endeavors of German scholars, and also how it has come about that in linguistic science a sort of metaphysics has arisen, which is at present undergoing a process of dissolution. But at the same time I wished at least to intimate that it is wrong to undervalue endeavors of this nature, since the occasion for such investigations is found in the linguistic material itself, and will probably continue in the future.

I would beg that my estimate of linguistic science and of the great philologists may be judged from this more general stand-point. It was not my intention to write a glorification of linguistic science, but to contribute toward a just estimate of it. My position with regard to the great philologists BOPP, GRIMM and others is as untrammelled as that we occupy to-

ward SHAKESPEARE and GOETHE. If a historian of literature asks whether GOETHE possessed dramatic talent in the highest and truest sense, no one will charge him with lack of reverence, but it will be recognized that he has only done his duty in proposing and answering this question. In the same sense I claim for myself the right to investigate what constitutes the actual power of that richly-endowed master to whom we owe the foundation of our science. Whoever reads with unprejudiced mind my sketch of BOPP and SCHLEICHER will, I hope, be impressed with the fact that my pen was guided by both love of truth and a feeling of veneration for these great men.

With the above remarks I would commend this book to the kind indulgence of the English and American public.

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## TRANSLATOR'S PREFACE.

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**W**hen I began to translate this little book, shortly after its publication, I did not anticipate the various delays and interruptions which have postponed the completion of my task for a whole year. So long a delay might be fatal to the usefulness of a translation, in the case of a work which aimed at a systematic exposition of the whole science of comparative philology, down to the latest development of its smallest detail, and the discussion of all disputed questions relative to both method and practice. A treatise with such an aim in view would require constant revision and extension, and would be completely antiquated in the course of a year. The present work, however, only proposes to exhibit the historical development of the science, and while discussing the chief problems which now present themselves in this field, it does not claim to chronicle all the various attempts to solve them, nor to initiate the reader into the intricate details of a philological warfare which is today raging at its hottest. Therefore this little volume may be said to fulfil its avowed purpose as well at present as it did a year ago.

I was impelled to undertake this translation by the consideration that I had never found a book which appeared to me to give so clear und succinct an account of the rise and development of comparative philology in Germany. It therefore

seemed particularly desirable that this condensed sketch of the progress in linguistic methods should be made accessible to those who are not conversant with German philological literature, more especially to those who are desirous of subsequently devoting more attention to the subject. If this translation shall serve to awaken or strengthen an interest in the science which owes its origin as well as many of its most able representatives to Germany, I shall feel amply repaid for any time and labor spent upon it.

A few words will suffice in explanation of the method I have pursued. My first aim was to render the sense of the German with the utmost possible accuracy, so that if I have erred, it has been on the side of too close adherence to the text of the original. In those instances where trivial alterations have been made, this has occurred with the knowledge and consent of the author, and the same is true of the very few notes I have ventured to add, which are always designated as the translator's. No one who has not made a similar attempt can realize the peculiar difficulties of transferring the German philological nomenclature to the English tongue, where certain of the technical terms, it is true, already have their recognized equivalents, but others are either differently rendered by different scholars, or are not represented at all in the language. In many cases where an important term could not be adequately translated, I have thought it only fair to introduce the German word in brackets. The titles of the German works quoted I have thought it more advisable to repeat in their original shape, since few of these works are translated, and for purposes of reference the German title would be necessary. Whenever Prof. WHITNEY has been quoted, I have naturally referred to his own book, instead of to JOLLY'S German translation, and similarly, the extracts from BOPP'S *Analytical Comparison* appear in their original English form, as

well as the remarks of Sir WILLIAM JONES on page 1. Two or three of the longer sentences from CURTIUS' *Grundzüge* have been quoted in the English translation, in which case the page of the translation has been added in square brackets. In my transcription of Sanskrit words I have adopted the method recommended by Prof. WHITNEY in the "Proceedings of the American Oriental Society" for Oct. 1880, and used by him in his Sanskrit Grammar, which forms the second volume of this series; for Zend, HÜBSCHMANN'S transcription, advocated by him in *Kuhn's Zeitschrift*, 24, page 328 seq., has been employed.

In conclusion, I must express my heartiest gratitude to Prof. DELBRÜCK for the cordial sanction he has given to my undertaking, and above all, to Prof. SIEVERS, who was so kind as to read over the whole translation, and to offer many valuable hints and suggestions.

Leipzig, Dec. 1881.

E. CHANNING.

## CONTENTS.

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### I. HISTORICAL PART, Chapters I—IV.

#### CHAPTER I: FRANZ BOPP, pages 1—26.

1. BOPP's views of the origin of inflection, 3—16. BOPP at first follows **FRIEDRICH SCHLEGEL**. Theory of the latter, 3—5. BOPP's theory in its first form, 5—9. BOPP's theory in its second form, 9—12. The third and final form, 12—15.

2. BOPP's method of comparing given languages, 16—25. Is BOPP's mode of view that of natural science? His general views of language, 17—20. BOPP's conception of phonetic laws: a) mechanical laws (i. e. law of gravity), 20—21; b) physical laws, 21—23. Incompleteness of BOPP's system of phonetics, — especially evident in his treatment of non-Indo-European languages, 23—24. Comprehensive estimate of BOPP, 25—26.

#### CHAPTER II: BOPP'S CONTEMPORARIES AND SUCCESSORS DOWN TO AUGUST SCHLEICHER, pages 26—40.

**WILHELM VON HUMBOLDT**, 26—28. **A. W. VON SCHLEGEL**, 28—32. **SCHLEGEL**'s position toward BOPP, 29. **LASSEN**'s critique, 30—31. **JACOB GRIMM**, 32—34. **A. F. POTT**, 35—36. **THEODOR BENFAY**, 36. Development of linguistic science down to **SCHLEICHER**, 37—40. Progress in knowledge, 37—39; in method, 39—40.

#### CHAPTER III: AUGUST SCHLEICHER, pages 40—55.

**HEGEL**'s influence, 40—42. Influence of natural science, 42—44. **SCHLEICHER**'s works, 44—45. Comparison between his views and BOPP's, 45—47. **SCHLEICHER**'s parent speech, 48—53. **SCHLEICHER** not a scientist, but a philologist, 54—55.

#### CHAPTER IV: NEW ENDEAVORS, pages 55—61.

New endeavors are manifest in the following tendencies: 1) Less interest is felt in the history of inflection (i. e. the origin of forms in primitive periods of the parent speech), 56. 2) It is recognized that no composition of unfinished linguistic elements can take place in the individual languages, 57. 3) More rigorous demands are made upon phonetic laws. The view arises that phonetic laws admit of no exceptions, 60. In consequence of this, attention is especially drawn to formations by analogy, 60. Importance of modern languages, 61.

## II. THEORETICAL PART, Chapters V—VII.

## CHAPTER V: THE AGGLUTINATION THEORY, pages 61—102.

Improbability of the other two theories which have thus far appeared: a) the evolution theory, in the form given it by WESTPHAL, 62—66; b) LUDWIG's adaptation theory, 66—71. BOPP's theory supported by the analogy of other languages, 71—73. Thus the agglutination theory is recommended in principle. A special discussion follows: 1) roots, 73—85; 2) the noun, 85—92; 3) the verb, 92—99. Can we distinguish different periods in the primitive speech? 100. ASCOLI's hypothesis, 100—101. Concluding observations, 101—102.

## CHAPTER VI: PHONETIC LAWS, pages 102—130.

Sketch of the views of G. CURTIUS. Three notions are prominent: 1) phonetic laws; 2) analogy; 3) the influence of sense on sound. These are discussed in inverse order:

1. Influence of sense on the preservation of sound, 106—107.

2. Action of analogy, 107—113. Demarcation of the field, 108. Classification, 108—111. Practical problems, 111—112.

3. Phonetic laws, 113—130. Definition of the notion "phonetic change" as such, 113—115. The regularity of phonetic change cannot be inductively proved, 115—117. Hence a solution is sought deductively, 117—129. What are the causes of phonetic change? — according to CURTIUS, WHITNEY, OSTHOFF, 117—120. More general theory, in agreement with BENFAY, 120. From this stand-point is shown: 1) how a language can split into dialects, 121—122; 2) how great a uniformity can be expected within the bounds of the dialect, 123—129. Fluctuations must be assumed for a state of transition, 123—124; but no permanent lack of uniformity in the treatment of sounds, 125—129. Comprehensive remarks on the notion and significance of phonetic laws, 129—130.

## CHAPTER VII: THE SEPARATION OF THE RACES, pages 130—139.

BOPP's view, 131—132. Theory of a genealogical tree, 132—134. "Wave-theory", 134—136. Present state of the question, 136—139.

## INDEX, pages 140—142.

## CHAPTER I.

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### FRANZ BOPP.

When FRANZ BOPP (born in 1791), the founder of comparative philology, began to devote his attention to Sanskrit, the statement that the language of the Brahmans was nearly related to the languages of Europe, especially to Latin and Greek, had been repeatedly made, and strengthened by a number of authentic proofs. Above all, Sir WILLIAM JONES, the first president of a society organized in Calcutta for the exploration of Asia, had, as early as 1786, expressed himself on this point as follows :

"The Sanskrit language, whatever may be its antiquity, is of wonderful structure; more perfect than the Greek, more copious than the Latin, and more exquisitely refined than either; yet bearing to both of them a stronger affinity, both in the roots of verbs and in the forms of grammar, than could have been produced by accident; so strong that no philologer could examine all the three without believing them to have sprung from some common source, *which, perhaps, no longer exists*. There is a similar reason, though not quite so forcible, for supposing that both the Gothic and Celtic, though blended with a different idiom, had the same origin with the Sanskrit." (Cf. BENFEY, *Geschichte der Sprachwissenschaft*, page 348.)

In the main coincident with the above, but less correct in one point, are the opening sentences of FRIEDRICH SCHLEGEL's celebrated book on the language and wisdom of the Hindus (*Ueber die Sprache und Weisheit der Indier*, Heidelberg, 1808):

"The old Indian *Sanskrito*, i. e. the refined or perfect, also called *Gronthon*. i. e. the written or book-language. bears the closest relationship to the Roman and Greek as well as to the Germanic and Persian languages. The resemblance is found not only in a large number of roots, which it has in common with them, but extends to the innermost structure and grammar. The agreement is therefore not an accidental one. such as could be explained through intermixture. but an essential one, which points to a common origin. On comparison it is further discovered that the Indian language is the elder, *the others younger and derived from it.*"

We cannot, therefore, say that BOPP was the discoverer of the Indo-European<sup>1)</sup> community of language, but to him is due the credit of having instituted a systematic comparison, which, starting from the forms of the verb, gradually extended over the whole language, and of thus demonstrating for all time what JONES, SCHLEGEL and others had only suspected or affirmed.

This demonstration will, without doubt, be regarded in the future as the epoch-making achievement of BOPP's genius, but it is quite as certain that BOPP himself from the very beginning had in view not the comparison, but the explanation of forms, and that comparison was to him only a means to the attainment of this chief end. To illustrate by an example: he was not satisfied with the discovery, so all-important for the phonetics of each individual language, that *ásmi*, *eiμi*, *sum*, *im*, *jésmi* are all at bottom one and the same form; but it was of greater interest to him to learn from what elements this form had arisen. Not a comparison of actual forms of speech, but an insight into the origin of inflection was the essential aim of his work.

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<sup>1)</sup> I have followed Prof. WHITNEY and others in preferring the term "Indo-European" to "Indo-Germanic", which latter name cannot in English claim the excuse of preponderating usage alleged by Prof. DELBRÜCK in support of its German equivalent. He says: "I use the name 'indogermanisch' (originated by KLAPROTH?) because, as far as I can see, it is the most common in Germany." The term "Aryan", so frequently employed by English philologists, I have rejected as being more properly applicable to the Indo-Iranian division of the family. [Translator.]

That this is really the case has been abundantly emphasized by the older as well as the more recent critics of BOPP. It will suffice here to recall the well-known statement of BOPP's teacher WINDISCHMANN, namely, that BOPP's aim from the beginning was "to penetrate by way of linguistic investigation into the mystery of the human soul, and to gain some cognizance of its nature and laws"; and to quote a remark of THEODOR BENFEY: "I would therefore consider that the real task of this grand work [the Comparative Grammar] was to gain a knowledge of the origin of the grammatical forms of the Indo-European languages; that their comparison was only a means to the attainment of this end, merely a method of discovering their fundamental forms; and that, finally, the investigation of phonetic laws was the chief means of comparison, the only sure foundation for the proof of relationship, especially of the fundamental forms."

Under these circumstances it seems to me expedient to speak first of BOPP's view of the origin of inflection, and afterwards to discuss his method of comparison.

#### I. Bopp's views of the origin of inflection.

BOPP's theories concerning the genesis of linguistic forms are not, as might be imagined, the pure result of his grammatical analysis, but can be traced back in great part to older views and prejudices. Among these the theory of FRIEDRICH SCHLEGEL, which is brought forward in his above-mentioned work *Ueber die Sprache und Weisheit der Indier*, plays an important part. It seems to me necessary, therefore, to familiarize the reader with this theory at the outset.

According to FRIEDRICH SCHLEGEL there are two chief classes of languages; first, those which characterize the minor shades of meaning by an inner change of the root, and secondly, those which for this purpose affix actual words having in themselves the significance of plurality, past time, future obligation, or other comparative notions of the sort. The first class embraces the inflectional languages. SCHLEGEL therefore understands by "inflection" the inner change of the root. He most emphatically opposes the view that the



inflectional forms could have been obtained by affixing previously independent words<sup>1)</sup> :

"In Greek there is at least a semblance of possibility that the inflectional syllables might have had their source in particles and auxiliary words which have melted into the word itself, although it would not be possible to carry out this hypothesis without having recourse to almost all the etymological artifices and juggleries which should all, without exception, be banished at the outset, if we are to view language and its origin scientifically, i. e. in a thoroughly historic light; and even then this hypothesis could scarcely be carried out. But in Sanskrit the last semblance of such a possibility vanishes, and we are compelled to admit that the structure of the language is a thoroughly organic one, ramified by inflections or inner changes and variations of the root in all its significations, and not a simple mechanical compositum formed by the affixion of words and particles, while the root itself remains barren and unchanged." (Page 41.)

In this organic nature he finds the main advantage of the inflectional languages :

"To this is due on one side the wealth, on the other the stability and durability of these languages, which can be said to have arisen organically, and to form an organic tissue; so that centuries after, in languages which are separated by broad tracts of land, it is often possible with little pains to find the thread which extends through the wide-spread wealth of a whole word-family, and leads us back to the simple origin of the first root. On the other hand, in languages which instead of inflection have only affixes, the same cannot be said of the roots; they are no fruitful seed, only a heap of atoms, as it were, which every chance wind can easily scatter or sweep together; the connection is really no other than a purely mechanical one, by means of outward affixion. In their first origin these languages lack a germ of living development" etc. (Page 51.)

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<sup>1)</sup> Probably in this opposition he has in mind the school of LENNEP and SCHEID (v. below), hardly HORNE TOOKE (concerning whom cf. MAX MÜLLER, *Lectures on the Science of Language*, page 255).

If we ask how this explanation of inflection as an inner change of the root, which seems to us so wanting in precision and clearness, can have arisen in the mind of this gifted scholar, so much is plain at once, that it was not derived from immediate observation (for where could we observe such an organic growth?): it seems more probable that it is really nothing but the necessary logical opposite of the theory which SCHLEGEL felt obliged to reject. In face of the absurdities of LENNEP, SCHEID & Co., by whom language was most stupidly cut to pieces and forcibly derived from purely imaginative roots, SCHLEGEL had evidently arrived at the conviction that it was impossible to approach the mystery of the development of linguistic forms by means of analysis. He therefore, in opposition to the theory which explained the origin of language by composition, preferred to postulate its development by means of organic growth, without very distinctly picturing to himself the nature and causes of this growth. He was perhaps strengthened in this view by another consideration. The relation existing between the Latin and Romanic languages (which his brother afterwards sought to characterize by the expressions "synthetic" and "analytic") seemed to him the more remarkable from the fact that in Sanskrit he found, so to speak, a more Latin condition of things than in Latin itself. (Page 40.) If, he may have concluded, a language shows the less composition the more ancient it is, how can we suppose that the linguistic forms in oldest times originated entirely by means of composition?

Now it was quite in the spirit of the philosophers of the Romantic School, with whose train of thought and method of expression SCHLEGEL was familiar, that he characterized such a growth from within outwards as "organic", and at the same time regarded this organic growth, in comparison with composition, as the higher and nobler process.

BOPP, in his first publication (*Conjugationssystem der Sanskritsprache*, 1816) adopted fully this briefly-outlined theory of SCHLEGEL (although without mentioning the author's name), which he afterwards stoutly opposed. But he extended it at once in one direction, by adding to the criterion of the

inner variation of the root the capacity to incorporate the substantive verb<sup>1)</sup> :

"Among all the languages known to us", he says on page 7, "the sacred language of the Hindus shows itself one of the most capable of expressing the most varied conditions and relations in a truly organic way, by *inner inflection and change of the stem-syllable*. But in spite of this admirable flexibility, this language is sometimes fond of incorporating into the root the abstract verb, whereupon the stem-syllable and the abstract verb share the grammatical functions of the verb."

This division of labor can be observed, for example, in the aorist, in the following manner. In the Sanskrit *āgrauṣam*, "I heard", *a* characterizes past time; the especial modification of the past which is peculiar to the aorist is intimated by the strengthening of the *u* in the root *gru* to *au*; and the substantive verb is incorporated into the thus formed preterit, "so that, after the time-relations have been expressed in a purely organic way by inner variation of the root, person and number are defined by inflection of the affixed auxiliary verb." (Page 18.) The incorporation of the substantive verb is supposed by BOPP to have taken place in the future and aorist in Sanskrit and Greek, in the Sanskrit precativē, in the well-known perfect and imperfect formations of Latin, and (although he afterwards gave this up) in the passive endings of the same language. BOPP recognizes no other composition than that with *as* in his *Conjugationssystem*. To be sure he speaks of affixing the "characteristics of person" [*Personskennzeichen*] M, S, T, but he does not recognize in these characteristics any remains of formerly independent words. On the other hand, he remarks expressly in another connection: "It is contrary to the spirit of the Sanskrit language to express any relation by affixing several letters which can be regarded as an individual word." (Page 30.) In the *Conjugationssystem* he leaves the origin of these "characteristics of person" just as much in the

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<sup>1)</sup> BOPP can have had this method of explanation alone in mind, when he says (*Conjugationssystem*, page 12) that in his labors he never leans upon the authority of another.

dark as the origin of the "interpolated" vowel *i*, which characterizes the optative.

It would be interesting to discover what considerations induced BOPP to modify SCHLEGEL's definition of the idea of inflection. Fortunately there is sufficient material for this in BOPP's writings. But in order to make the passages in question intelligible, I must first say a word about the customary classification of the parts of speech at the beginning of our century. There was at that time a general prejudice in favor of the theory that the sentence must be an image of the logical judgment; hence arose the opinion that, inasmuch as a judgment consists of three parts, subject, predicate and copula, the number of the parts of speech also must be neither larger nor smaller than three. It was naturally no easy matter to bring the traditional parts of speech under three heads, and this classification could not be carried out without sophistry. For instance, A. F. BERNHARDI knew no better method of reconciling his philosophical theory with his practical experience than by making out the following table:

I. Parts of speech [*Redetheile*]:

- a. Substantives.
- b. Attributives.
  - aa. Adjectives.
  - bb. Participles.
  - cc. Adverbs.
- c. The verb *be*.

II. Smaller parts of speech [*Redetheilchen*]:

- a. Prepositions.
- b. Conjunctions.
- c. Original adverbs.

III. Parts of speech and smaller parts of speech:

Pronouns.

GOTTFRIED HERMANN is convinced, as well as BERNHARDI, that there can be but three parts of speech, and we find that BOPP was of the same opinion, as most clearly appears

from a remark in the English version of his first publication, *Analytical Comparison*<sup>1)</sup> etc., page 14: "*Potest* unites in itself the three essential parts of speech, *t* being the subject, *es* the copula and *pot* the attribute." Here it is especially noteworthy that not the verb as such, but only the verb *be* is regarded as the third part of speech.

GOTTFRIED HERMANN says (*De emendanda ratione graecae grammaticae*, Leipzig, 1801, page 173):

"Est enim haec verbi vis, ut praedicatum subjecto tribuat atque adjungat. Hinc facile colligitur proprie unum tantummodo esse verbum idque est verbum esse. Caetera enim quaecunque praeter hoc verbum verba reperiuntur, hanc naturam habent, ut praeterquam quod illud esse contineant, quo fit ut verba sint, adjunctam habeant etiam praedicati alicujus notationem. Sic 'ire' 'stare', ut aliqua certe exempla afferamus, significat 'euntem, stantem esse'."

BOPP shared this opinion, as is sufficiently clear from the first words of his *Conjugationssystem*, which are as follows:

"By the word 'verb' in its strictest sense is meant that part of speech which expresses the union of an object with a quality, and their relation to each other. According to this definition, the verb has in itself no actual significance, but is simply the grammatical bond between subject and predicate, through whose inner variation and change of form these mutual relations are indicated. In this sense there is but one verb, viz., the abstract verb, *be*, *esse*" etc.

Since, therefore, according to BOPP's view no predicate can exist except with the aid of the verb *esse*, and since, accordingly, this predicate in point of meaning inheres in every so-called verb, to be consistent BOPP would necessarily find it natural that the verb *as* should be palpably and visibly represented in every verbal form. BOPP did actually accept this consequence in a very remarkable sentence in the *Analytical Comparison*, page 14:

<sup>1)</sup> *Analytical Comparison of the Sanskrit, Greek, Latin and Teutonic Languages, showing the original identity of their grammatical Structure*, by F. BOPP, published in the *Annals of Oriental Literature*, London, 1820. This is not merely a translation, but in many respects quite different from the German original. [Transl.]

"After these observations the reader will not be surprised if, in the languages which we are now comparing, he should meet with other verbs constructed in the same way as *potest*, or if he should discover that some tenses contain the substantive verb, *while others have rejected it or perhaps never used it*. He will rather feel inclined to ask: 'Why do not all verbs in all tenses exhibit this compound structure?'—and the absence of the substantive verb he will perhaps consider as a kind of ellipsis."

Whoever carefully weighs this extraordinary sentence, in which the solution of a difficulty is ingeniously thrown upon the reader, while he would naturally expect the author to solve it, will certainly agree with me when I assert that BOPP was led to seek the substantive verb in the occasionally appearing *s* of Indo-European forms, chiefly in consequence of his false theory concerning the three parts of speech.

Accordingly, we can characterize BOPP's oldest theory of inflection, as we find it in the *Conjugationssystem*, as the union of an *aperçu* of SCHLEGEL with the traditional theory of the three parts of speech.

In the above-mentioned English version (1819), the *Analytical Comparison*, we find a very marked advance upon the view brought forward in the *Conjugationssystem* (1816). This progress can be briefly summed up as follows: the principle of composition, which up to this time was only applied in the case of the root *as*, is now recognized as the prevailing one. How BOPP arrived at this change of opinion can be traced out in his explanation of the notion "root", and in his hypothesis concerning the origin of the personal endings of the verb.

First of all, in regard to the notion "root", it was possible for BOPP to derive from the grammatical tradition prevalent at his time the opinion which he here expressed and retained ever after, namely, that all words go back to *monosyllabic* elements. For ADELUNG had already declared that all the words of the German had their origin in monosyllabic constituent parts, which bear the name "root". (Cf. ADELUNG, *Ueber den Ursprung der Sprache und den Bau der Wörter*, be-

*sonders des Deutschen*, Leipzig, 1781, page 16 seq.)<sup>1)</sup> BOPP found this view confirmed by an investigation of the Sanskrit root-indices, with which he became acquainted in the edition of CAREY and WILKINS. (Cf. A. W. von SCHLEGEL, *Indische Bibliothek*, 1, 316 and 335.) He formulated his opinion in the *Analytical Comparison*, page 8, as follows:

"The character of Sanskrit roots is not to be determined by the number of letters, but by that of syllables, of which they contain only one; they are all monosyllabic, a few excepted, which may justly be suspected of not being primitives." (Cf. also A. W. von SCHLEGEL in the above article, page 336.) Now BOPP assumed for the roots of the kindred languages what was true of the Sanskrit roots, and accordingly made the statement: "Roots are monosyllables in Sanskrit and its kindred languages."

With this conception of "root", SCHLEGEL's idea of inflection must naturally appear very questionable. For how can a monosyllabic root (especially if, as is obviously the case, the consonants remain intact) be inwardly inflected and altered to any considerable extent? The idea of the monosyllabic nature of the root must necessarily strengthen that of composition in inflection, and it is therefore not surprising that BOPP's polemic against SCHLEGEL had its starting-point just here. We find this polemic expressed in the following paragraph, page 10:

"If we can draw any conclusion from the fact that roots are monosyllables in Sanskrit and its kindred languages, it is this, that such languages cannot display any great facility of expressing grammatical modifications by the change of their original materials, without the help of foreign additions. We must expect that in this family of languages the principle of

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<sup>1)</sup> It is not uninteresting to see what was the doctrine of FULDA, a predecessor of ADELUNG (*Sammlung und Abstammung germanischer Wurzelwörter*, Halle, 1776), concerning the method of obtaining roots:

"Take from a single word its grammatical functions, its prefixes and suffixes, verbal, nominal, and those of gender, number, case, person, tense. Wherever at the beginning or end two consonants stand together, cast away the foremost and hindmost; the *root*, without losing any of its chief significance, will become a single syllable." (Page 59.)

compounding words will extend to the first rudiments of speech, as to the persons, tenses of verbs, and cases of nouns &c. That this really is the case, I hope I shall be enabled to prove in this essay, in opposition to the opinion of a celebrated German author, who believes that the grammatical forms of the Sanskrit and its kindred languages consist merely of inflections, or inner modifications of words."

The second point is still more important, viz., the hypothesis which appears in the *Analytical Comparison* concerning the derivation of the personal suffixes from personal pronouns. The passage where this hypothesis is first introduced is so interesting that I quote it in full:

"The indication of the persons of verbs in the Sanskrit language and those of the same origin Mr. F. SCHLEGEL considers as being produced by inflection; but SCHEIDIUS shews very satisfactorily, with respect to the plural at least, that even the Greek verbs make use of pronouns, in compound structure with the root, to indicate the various persons. With respect to the singular, he would have succeeded much better if he had not limited himself to the corrupt form in  $\omega$ , terminating the third person of the present in  $\alpha$ , where I cannot perceive any pronoun incorporated, — but had extended his view to the form in  $\mu$ , terminating the third person in the Doric dialect with  $\tau$ . SCHEIDIUS commits another fault, namely, that in speaking of the pronouns he stops at the nominative, whilst the crude form of nouns may be better extracted from the oblique cases. In this way it is easy to discover that  $\tau\omicron$  is the radical form of the Greek article, which is originally nothing more than a pronoun of the third person, and is used as such in Homer. This  $\tau\omicron$ , bereft of the final vowel, becomes an essential element of verbs in their third person, singular, dual and plural, as  $\delta\acute{\iota}\delta\omicron\tau\iota$  (!),  $\delta\acute{\iota}\delta\omicron\tau\omicron\nu$ ,  $\delta\acute{\iota}\delta\omicron\nu\tau\iota$ . I have no doubt but it can be proved, with as much certainty at least as in the case of the Arabic, that Sanskrit verbs also form their persons by compounding the root with the pronouns, upon which subject I shall offer a few remarks in its proper place." (Page 11.)

For these intended remarks, however, BOPP seems to have found no opportunity in the course of his discussion, and merely observes (page 16): "In the present tense the pronominal



consonants M, S, T, of the singular number and of the third person plural, are articulated with a short *i*”, — from which we see that at that time he had not come to the conclusion, as he did later, that *mi* arose from *ma*.

In the above exposition our attention is first of all attracted by the reference to SCHEIDIUS, who is said to have already established the principle of composition “very satisfactorily”. He refers to the detailed treatment of the question contained in *L. C. Valckenarii observationes acad. et Jo. Dan. a Lennep praelectiones academicae rec. Everardus Scheidius (Trajecti ad Rhenum, 1790)*, page 275 seq. Leaving it to the reader to enjoy the various etymological fantasies, I will only quote the words of SCHEID which are of interest for the main question. They are as follows :

“Memini equidem, quum ante hos octodecim, et quod excurrit, annos, contubernio fruerer viri summi, quem honoris causa nomino, Joannis Jacobi Schultensii, inter familiares sermones, quibus de linguarum indole agebatur, narrare Schultensium, virum suavissimum et harum rerum elegantissimum arbitrum, Lennepio placuisse, ut. quemadmodum in verbis orientalium, adformantes, quae dicuntur. temporis praeteriti proprie essent syllabae literaeve, a pronomibus antiquis quasi resectae: ita et in Graecorum verborum temporibus personisque eadem fuisset sermonis ratio.”

We see from this passage that BOPP's view of the personal endings was finally suggested by Hebrew grammar.

Now that the principle of composition was once recommended in this way, it is no wonder that it was also applied in other cases than in the tenses compounded with *as*, and in the personal suffixes, — so, for instance, in the optative, whose *i* is first explained in the *Analytical Comparison*, page 23, as the verb “wish”, “desire”. Of real inflection in SCHLEGEL's sense of the term BOPP in the *Analytical Comparison* retains only certain vowel-changes (so the *ai* of the middle voice, which he did not then explain by means of composition, as he did later), and reduplication. (Pages 12 and 34.)

After BOPP's view had been formulated in the two ways above mentioned, in the *Conjugationssystem* and the *Analytical*

*Comparison*, it assumed at length a third and final shape, which was first introduced in a series of academical essays, and at last appeared in the *Comparative Grammar*, and which chiefly differs from the second form in more and more exclusively emphasizing the principle of composition, as well as applying it to those departments of grammar which had not been treated in the *Conjugationssystem* and the *Analytical Comparison*.

This theory is now intelligible without further preface, and we can sum it up briefly as follows:

The words of the Indo-European languages must be derived from *roots*, which are all monosyllabic. There are two classes of roots, viz., verbal roots, from which spring verbs and nouns, and pronominal roots, from which pronouns, primitive prepositions, conjunctions and particles have their origin. (Cf. beside the *Vergleichende Grammatik*, § 107, also *Abhandlungen der Berliner Akademie*, 1831, page 13 seq.)

The *case-endings* are at least for the most part<sup>1)</sup> pronouns by origin. Thus the *s* of the nominative is derived from the pronoun *sa*; the *m* of the accusative recalls the Sanskrit pronominal stem *i-ma*; the *T*-sound of the ablative comes from the same pronominal stem *ta* to which the neuter *d* in *id* also owes its origin, etc. (Cf. *Abh. der Akad.*, 1826, page 98.)

The *personal endings* of the verb are derived from the pronouns of the first, second and third person; *mi* is a weakening of the syllable *ma*, "which in Sanskrit and Zend forms the underlying theme for the oblique cases of the simple pronoun". From *mi* is further derived *m*. In the plural ending *mas*, is found either *as*, the plural characteristic of nouns, or the pronominal element *sma*. The *v* of the dual is only a corruption of the plural *m*. The endings of the second person in similar fashion go back to *tva*, those of the third person to *ta* (for *nti* v. below, page 15). BOPP does not express a confident

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<sup>1)</sup> "For the most part", because a few endings (*os* and *sām*) are not considered as accounted for, and sometimes a symbolical explanation (v. below, page 15) is attempted.

opinion in regard to the middle endings, but he thinks it probable that they are due to the doubling of the corresponding active endings.

As for the *characteristics of the present stem*, like *vo* in ζεύνομι, it is most probable that the greater part of these are pronouns.

The *augment*, which is mentioned in connection with the imperfect, is considered by BOPP (*Vgl. Gr.*, § 537, and even earlier in the *Analytical Comparison*, page 27) as identical with *a* privative, and is therefore regarded by him as a negation of present time. But he also admits the possibility of connecting it directly with the pronominal stem *a* "that"; to which, moreover, he regards the negative particle itself as related.

In the *S-aorist* the *s* belongs to the substantive verb, and the explanation of the composition is that the imperfect of *as* (but without the augment) forms the end of it. "I recognize", he says in § 542, "in this *s* the substantive verb, with the imperfect of which the first form [of the aorist] wholly coincides, except that the *ā* of *āsam* etc. is lost". The *sya* of the *S-future*, such as *dāsyāti*, BOPP regards as the future of *as*, which is lost in its isolated use. Beside this, he thinks it probable that all verbs once possessed a future formed by means of *ya*, and that this *ya* itself, as well as the sign of the optative, comes from the root *i* "wish".

In the *aya* of *causatives* he discovers the verb *i* "go" (as well as *yā* "go" in the *ya* of the Sanskrit passive), and in the *s* of *desideratives* the substantive verb.

The same composition is met with in certain *formations of the individual languages*, e. g. *ama-vī*, in which the root *bhū* can be recognized; *ama-rem*, where we find the root *as*, etc.<sup>1)</sup> (Cf. *Vgl. Gr.*, § 521.)

Finally, the *stem-forming suffixes* are partly of pronominal, partly of verbal origin (e. g. *dātar* "giver" means really "he who walks through the action of giving", from *dā* "give" and *tar* "walk through").

<sup>1)</sup> On the other hand, BOPP does not assume that new root-words could arise in an individual language. (Cf. preface to the third section of the *Vgl. Gr.*, 1<sup>st</sup> edition, page XIV.)

Beside this explanation by composition, a second is sometimes brought forward, the *symbolical*. Thus the following remark is made concerning the dual: "The dual, inasmuch as it is founded upon a clearer observation than the more indefinite plurality, prefers the fullest endings, as conducing to stronger emphasis and a more vivid personification." (*Vgl. Gr.*, § 206.) The same is true of the feminine, "which in Sanskrit prefers a luxuriant fullness of form, in the stem as well as in the case-endings." (§ 113.) The *n* is also symbolic in the third person plural *-nti*, which is supposed to be formed from *ti* by the insertion of a nasal. He considers that this insertion is the least strange of admixtures, and the nearest approach to the simple lengthening of an already existing vowel. (§ 236; cf. also § 226.)

If we compare this final aspect of BOPP's views with the preceding one, we observe that SCHLEGEL's influence has dwindled down to a slight remnant. For the *ai* of the middle endings, in which BOPP formerly saw an inner inflection of the root, is now rather explained by composition, and therefore reduplication alone remains as a sort of inner modification of the root. (And even this reduplication, which perhaps was originally the repeated root, cannot be called an "inner" change in any strict sense of the word.)

Accordingly it was natural that BOPP should formally declare his disagreement with FR. SCHLEGEL, by a keen polemic in the Comparative Grammar. The passage referred to is as follows:

"By 'inflection' FR. v. SCHLEGEL understands the inner change of the root-sound, or the inner modification of the root, to which he opposes affixion from without. But if the Greek δίδωμι, δάσσω, δοθῆσόμεθα come from δο or δω, what else are the forms μι, σω, θησόμεθα except evident external additions to the root, which in its interior is either not changed at all, or only in the quantity of the vowel? If, then, we are to understand by 'inflection' an inner modification of the root, Sanskrit, Greek etc. scarcely exhibit any inflection at all, with the exception of reduplication, which is derived from the resources of the root itself. If, on the other

hand, *θησόμεθα* is an inner modification of the root *δο*, simply because it is connected with it, is adjacent to it, and together with it represents a whole, then the notion of sea and main land could as appropriately represent a modification of the sea, or the reverse."

We can characterize the theory of BOPP, as developed above, leaving out the slight symbolical addition, as the *composition* or *agglutination* theory<sup>1</sup>).

I will not attempt here a more detailed criticism of the agglutination theory, but will leave it for the fifth chapter. I would like, however, to call attention again to the fact that BOPP's explanations have not, as has been supposed, spontaneously arisen as the natural consequence of comparison, but that they have grown out of various and independent views and conclusions. For in addition to the suggestive stimulus resulting from the details of the investigation itself, BOPP had also in mind bits of the learned tradition of former times, as for instance the prejudice in favor of the threefold nature of the parts of speech, which seems to have first given rise to the idea that the substantive verb is to be recognized in the shape of various *s*'s in the verbal forms; further, the transmitted theory that roots are to be regarded as monosyllabic; and finally, the tradition derived from Hebrew grammar, that we have to recognize affixed pronouns in the personal suffixes of the verb.

## II. Bopp's method of comparing given languages.

Having discussed BOPP's theory of inflection in the first section of this chapter, I will now treat of his comparison of given individual languages. Of course it cannot be my aim to record the results which have been attained through BOPP's comparison of the Indo-European languages; I will simply attempt to describe the method which BOPP employed.

We must not, however, expect from BOPP a systematic answer, which shall comprehend all separate instances, either on this point or any other. BOPP's method of demonstration is

<sup>1</sup>) It was so named first by LASSEN, with the intention of casting a slur upon it. (Cf. POTT, *Etymologische Forschungen*, 1<sup>st</sup> edition, 1, 179.)

exactly the opposite of HUMBOLDT's. While WILHELM VON HUMBOLDT is never weary of expounding generalities, and exerts himself at every turn to subordinate details to ideas, BOPP occupies himself chiefly with individual points in language, and very seldom intersperses general observations, such as could be termed "philosophical". It is as impossible to obtain a theory and systematic method for linguistic science from BOPP's Comparative Grammar, as it would be to extract grammatical paradigms from HUMBOLDT's "Introduction to the Kawi Language". Under these circumstances we must investigate with caution BOPP's theoretical views in regard to the forces at work in language, — that is, we must be careful, where he uses certain terms with an easy carelessness, not to examine their significance and breadth of application as inexorably as if we were setting up a system of terminology. I feel, therefore, as if the fairest way of proceeding were to frame our question thus: what are the general views constituting the standpoint from which BOPP was accustomed to judge the processes of language? — and to answer the question as follows: his general views had a coloring of natural science, beneath which, however, the old philological background had not yet vanished. His fondness for the terminology of natural science is at once apparent when he attempts to describe his method of treating language in contrast to that of former scholars. He aims at a comparative "dissection" [*Zergliederung*] of language; systematic comparison of languages is a "language-anatomy"; we have to deal with an "anatomical dissection" or "chemical decomposition" of the body of language, or, to use another figure, with the "physics" or "physiology" of language. This coloring is very prominent in the first sentence of the preface to the *Vergleichende Grammatik*:

"In this book my aim is a comparative, comprehensive description of the organism of the languages mentioned in the title, an investigation of their physical and mechanical laws, and the origin of the forms characterizing grammatical relations."

What is meant by "physical and mechanical laws" in this sentence, the author has himself explained in reply to inquiry, as BRÉAL informs us in the French translation of BOPP's Com-

parative Grammar. By "physical laws" is meant what we now call "phonetic laws"; by "mechanical laws" the rules which BOPP believed he had established concerning the relative weight of vowels and syllables, of which we shall speak later. The meaning of "organism" and "organic" is shown by one or two passages in the *Vergleichende Grammatik*. In the preface to *Heft 2*, 1<sup>st</sup> edition, page VII, we read: "The inflections make up the true organism of a language"; and on the other hand he speaks of "languages with monosyllabic roots, without the capacity of composition, and hence without organism, without grammar." (§ 108.) "Organism" of a language is accordingly nothing but the grammatical "arrangement" [*Einrichtung*], which is founded on agglutination (preface to the first volume of the *Vgl. Gr.*, page IV); "organic" is everything which is in accordance with this arrangement, and "inorganic" what is at variance with it. We can therefore say "original" instead of "organic", and "not original" instead of "inorganic". So, for example, the  $\nu$  of the ending  $\mu\tau\nu$  is said to be "organic, i. e. not a later, meaningless addition, but intentionally employed, and inherited from the primitive period of our branch of language"; on the contrary, the  $\mu$  of  $\tau\acute{o}\pi\tau\omicron\iota\mu$  is considered "inorganic", because the optative, in all languages where it exists as a separate form, has the short endings, even in the first person, with the single exception of Greek. Everything is "inorganic" which cannot, according to the view of the grammarian in question, be derived from the original structure of the Indo-European.

We see that the terms "mechanical", "physical", "organic" are not used strictly in the sense they possess in natural science, yet we can conclude from their application that BOPP's conception of language was of a kind of organic body. He uses this very word in the *Vocalismus*, page 1:

"Languages must be regarded as organic bodies [*organische Naturkörper*], formed in accordance with definite laws; having a life-giving principle within, they develop and then gradually die out, after losing consciousness of their true nature, and throwing aside, or mutilating, or misusing (i. e. applying to uses to which they were not primarily adapted) their

members or forms, which were originally significant, but have gradually become a more external mass."

This sentence introduces us to two new trains of thought. In the first place, I would call the attention of the reader to the remark that language in the course of time loses consciousness of its own nature. Here a mental activity is ascribed to language; it is referred to as if it were a thinking being. Nor is this an isolated instance. In other passages BOPP speaks of the spirit or genius of language, and recognizes in its procedure certain tendencies and aims. Sometimes, instead of language as a whole, an individual form is regarded as a thinking being. So for example in the *Vgl. Gr.*, 1<sup>st</sup> edition, page 516, the Slavonic stem *sjō* is said to be "no longer conscious of its composition, which was handed down from the primitive period of the language." These expressions are metaphors, — very natural ones, too, and probably, if any one had called his attention to the point, BOPP would have acknowledged that in reality these psychical activities take place, not in language, but in speaking individuals; yet it is important to call attention here to the first beginnings of a mode of view which with SCHLEICHER rose to a conscious hypostasizing of the notion "language".

In the next place, in the sentence above quoted the expression "die out" is noteworthy. According to BOPP, all external changes which we observe in the Indo-European languages betoken not development, but disease, mutilation and decline. We become acquainted with languages, not in their ascending development, but after they have passed the goal set for them. That is, we find them in a state "where they might still perfect themselves syntactically, but where, grammatically considered, they have lost more or less of what belonged to that perfect arrangement, in virtue of which the separate members were in accurate proportion to each other, and all derivative formations were still connected, by a visible and unimpaired bond, with that from which they originated." (*Vocalismus*, page 2.) As long as the meaning of the composition continues to be felt in a grammatical form, it offers opposition to any change. But the farther languages are separated from their source, the more love of euphony gains



in influence. (*Abh. der Berl. Akad.*, 1824, page 119.) This view has also been extended and systematized by SCHLEICHER.

Having thus briefly characterized BOPP's fundamental views, I will now give a more detailed account of his ideas concerning changes in language, and will classify them in accordance with the categories introduced by BOPP himself: *mechanical* and *physical laws*.

The effect of BOPP's so-called "*mechanical laws*" is especially visible in the changes which the weight of the personal endings produces in the stem. A light ending follows a heavy form of the stem, e. g. *émi* "I go", from *i* "go"; but before a heavy ending a light stem-form alone is permitted, e. g. *imás* "we go". The same law accounts for the German *Ab-laut*, which is preserved to the present day in *weiss* and *wissen*. These facts, which were first formulated by BOPP, we now explain in a different manner, by ascribing the weakening of certain syllables no longer to a law of relative weight, but to the power exercised by the accent of the following syllable.

Beside the influence of the weight of the personal endings, BOPP recognizes another action of this law of gravity, which will be apparent from the following examples. It is the task of the stem-syllables to carry the formative syllables, and it sometimes happens that a stem-syllable is not strong enough for this purpose. We have such an instance in the Sanskrit imperative *cinú* "gather", from *ci*; BOPP here remarks that the sign *nu* is only able to carry the ending *hi* when the *u* is supported by two preceding consonants, as for example in *āpnuhi*. "But where the *u* is only preceded by a single consonant, it has become incapable of carrying the ending *hi*, hence *cinú* 'gather', from *ci*." (§ 451.) In a similar manner BOPP explains the circumstance that the perfect endings appear greatly mutilated in comparison with those of the present. Since in the perfect the root has also the reduplication-syllable to carry, it is, so to speak, claimed by both sides at once, and is therefore no longer in a condition to lift a heavy ending. It is clear that this second law of gravity, whose action BOPP discovers in several other instances, is in direct contradiction to the first, and it

is now universally acknowledged that the idea expressed in this law suffers from a metaphorical obscurity.

I have intimated above that the mechanical laws can no longer be understood and accepted by us in the same manner as by BOPP, and will pass to the "*physical laws*", which we are now accustomed to call "*phonetic laws*". In order to appreciate BOPP's stand-point in this connection, it is important to come to a clear understanding of the possible method of establishing phonetic laws. Whoever compared Sanskrit with another Indo-European tongue, the Greek, for instance, was of necessity impressed with the fact that there exist in both languages words and formations which completely coincide. No one could avoid noticing, for example, that the Skr. *mātār* and Gr. μήτηρ, Skr. *dāma* and Gr. δόμος, Skr. *pītār* and Gr. πατήρ were the same words, and that the inflectional endings of the verb agree in the main in the two languages. The recognition of this agreement rested upon immediate evidence, and could not be further demonstrated. From comparison it was possible to deduce the rule that certain sounds of the Greek corresponded to certain sounds of the Sanskrit, *m* to *μ*, *t* to *τ*, etc. Yet after collecting a very few words, it immediately became plain that the same sound of the Sanskrit was not always represented by the same sound of the Greek. So for example in *dāma* δόμος, *dādāmi* δίδωμι, the Greek δ corresponded to the Sanskrit *d*; but in the pair *duhitār* θυγάτηρ, which no one wished to separate, the Sanskrit *d* was represented by a Greek θ. As a result of such observations, it was necessary to adopt the conclusion that these rules admit of exceptions, and to say accordingly: "Usually Sanskrit *d* corresponds to a Greek δ, but often also to a Greek θ." Now two positions are conceivable in relation to such a rule. We can either start with the theoretical conviction that laws admit of no exceptions, and feel ourselves bound to investigate the causes which produce the so-called "exceptions"; or we can content ourselves in the wording of our rules with the expressions "usually" and "often". And this latter is on the whole BOPP's stand-point. "We must expect to find no laws in language", he remarks, "which offer more resistance than the shores of rivers and seas". (*Vocalismus*, page 15.) In other passages he

adopts the same convenient view, at least for part of the phonetic processes observed by him, his opinion being that there are two sorts of euphonic change in language; "one, which is elevated to a universal law, appears in like form on every like provocation, while the other, which has not become a law, occurs only occasionally." (*Vgl. Gr.*, 1<sup>st</sup> edition, § 236, note.) That the latter class of phenomena in BOPP's opinion covers a broader ground than the former, is soon evident. He frequently claims for language the right to depart from the existing law with "a certain freedom". That vowels should be lengthened without cause, extensive mutilations take place without conceivable provocation (as for example that ἐτύπην should be a mutilated form of ἐτύφθην), and that the same phonetic group should pass into widely differing formations in the same linguistic period, appears to him not at all extraordinary. For instance, he assumes that the pronominal stem *sma* in Gothic appears in six different forms, as *nsa*, *sva*, *nka*, *nqva*, *mma* and *s*. (§ 167.) When he was unable to find in the same language an analogy for a phonetic change which seemed probable to him, he had recourse to another; for example, in order to confirm the assertion that the *l* of the Slavonic participles was derived from *t*, he referred to the Bengali. The *x* of δέδωχα he traces back to an *s*, but in τέτοφα this *x* has become *h*, "as it were in the spirit of the Germanic Law of Permutation of Consonants [*Lautverschiebungsgesetz*]", and this *h* together with the preceding tenuis or media has become an aspirate. (§ 569.) Even the admission of wholly isolated cases of change does not terrify him. BOPP but seldom claims infallibility for a phonetic law. An interesting example of the sort occurs in his article on the demonstrative pronoun and the origin of case-signs. (*Abh. der Berl. Akad.*, 1826.) There it is of great importance to him to prove that the article *sa*, *ó*, can never have had a nominative *-s*, and while rejecting the assumption that the *s* could have fallen away in Sanskrit and Greek, he adduces the infallibility of certain phonetic laws in the following expressive terms:

"But we must not overlook the fact that such eliminations [*Abschleifungen*] usually, if not always, occur in numbers and according to rule, rather than in single instances and arbi-

trarily; and if the spirit of a language at any period of its history conceives a hatred for any letter as the terminal pillar of a word, it removes it wherever it occurs, so that not a single such letter remains to give ground for the supposition that a similar one ever existed. In this way a phonetic law raged in Greek against the letter  $\tau$ , and eradicated it in every case where it stood as 'final consonant, in spite of the importance and extent of its grammatical rôle, which we can clearly recognize by comparison with the kindred languages. On the other hand,  $\sigma$  has always been a favorite final letter to Greek ears, and as readily as it has allowed itself to be dropped out in the middle between two vowels, just so persistently does it appear at the end, wherever the researches of comparative philology lead us to expect it."

We see from these quotations, which could be increased *ad infinitum*, that BOPP did ascribe infallibility to a phonetic law in single cases, where the facts seemed to prompt it, but by no means as a general rule; on the contrary, he granted to language the freedom of occasionally emancipating itself from the existing laws. It is universally acknowledged (even by those scholars who do not advocate the principle that phonetic laws admit of no exception) that BOPP has left the greatest task for his successors in the department of phonetics. The impression that the words compared were identical was, as already intimated, always decisive for him, and the sounds had to adapt themselves to this impression; in his assertions about sounds, he did not give sufficient weight to the modifying influence arising from a comparison of the fate of the same sounds witnessed elsewhere. It is to AUGUST FRIEDRICH POTT that the great credit is due of having filled up this gap.

This want of method in BOPP's investigations was not so palpably evident in the Indo-European domain, because there a great number of forms and words really exist in which the same sound appears in the same position, and because in the discovery of hidden resemblances BOPP was guided with wonderful correctness by the deep insight of his genius; it became very conspicuous, however, when BOPP undertook to introduce into his comparison languages whose relationship

to our linguistic branch was not established, — I refer to the Malay-Polynesian. I think it is now universally acknowledged by philologists that these languages have nothing in common with the Sanskritic languages, but BOPP was under the impression that they stood in a daughterly relation to the Sanskrit, and attempted to establish this relationship in the same way as he had that of the Indo-European languages in his Comparative Grammar, — so far, that is, as was permitted by the character of these tongues, which "have undergone a total dissolution of their original structure". Here, also, he formed no tables of phonetic correspondence, but compared words which seemed to him identical (e. g. numbers), and tried to account for the phonetic changes in each separate instance. His method was naturally more arbitrary here, where he had to work with an entirely antagonistic material, than within the Indo-European domain. As an example, I will take the word *po*, which has the meaning "night". BOPP says in regard to it (*Ueber die Verwandtschaft der malayisch-polynesischen Sprachen mit den indisch-europäischen*, *Abh. der Berl. Akad.*, 1840, page 172):

"The usual appellation of 'night' in the South Sea languages, i. e. in the New Zealand, Tahiti and Hawaiian tongues, is *po*; which, echo-like, reproduces only the last syllable of the Sanskrit *kṣapas*, *kṣapo*."

Now there is another word *bo* "day", which, as he says on page 218, might have been derived from the Sanskrit *divas*, *dīvo*. "But if", BOPP continues, "there should prove to be a connection between the Tongan *bo* and the above-mentioned *po*, which in the South Sea languages signifies 'night', we should be obliged to give up connecting this *po* with the Sanskrit *kṣapas*, and to assume that this *po* has lost an epithet which in the Tongan language changes 'day' to 'night', and characterizes the latter as 'black' or 'dark day'."

After what I have said of BOPP's relation to phonetics, it is not necessary to occupy ourselves any further with such vagaries, as it will be clear from the preceding that the failure of this undertaking in the field of the Malay-Polynesian does not manifest a constitutional blemish in linguistic

science as a whole, but simply a lack in Bopp's method, which was subsequently supplied.

Yet it was very natural that Bopp's ideas concerning phonetic change and phonetic laws should be rather latitudinarian. BOPP was no natural philosopher, but a philologist, who was occupied with grammars his whole life long. To a natural philosopher, it is true, the idea that a law can have exceptions at will is ridiculous or repugnant; but this view was quite common in philological theory and practice. In all grammars the mass of "irregular" words was at least as great as that of the "regular" ones, and a rule without exceptions actually excited suspicion. Such traditional opinions, indeed, only die out in the course of generations.

What BOPP achieved, as already remarked, was the establishment of an independent theory concerning the origin of inflection, and the scientific demonstration of the original community of the Indo-European languages.

Now that we have introduced the reader to BOPP's labors in both fields, we are able to declare briefly and comprehensively what mental peculiarity is especially prominent in the writings of this great scholar.

When we hear that a single individual has treated comparatively the Sanskrit, Old Persian, Zend, Armenian, Greek, Italic, Celtic, Slavonic and Germanic languages, and has even passed on beyond this immense field to the languages of the South Sea, we are easily inclined to ascribe to him an unusual, nay, an extraordinary amount of learning. But on a nearer examination we readily see that learning is not really a quality which is especially characteristic of BOPP. He certainly learned a great deal in the course of an industrious life, but he was not one of the men whose learning appeals to us as in the case with A. W. V. SCHLEGEL. He possesses from a philological point of view but scanty knowledge of many languages in the elucidation of which he acquired immortal honor, as for example the Slavonic and Celtic; and with regard to

certain traditional details, as for instance the rules of Latin grammar, he was occasionally more indifferent than could be desired. For example, he had no objection to giving his Sanskrit dictionary the title: "*Glossarium sanscritum a Franzisco Bopp*", and preferred to construe *postquam* with the pluperfect! Whatever did not seem to him to contribute to the explanation of forms and the comprehension of the primitive condition of language was comparatively indifferent to him.

Nor is it wholly correct that BOPP, as is often asserted, invented the *method* of linguistic comparison. BOPP is incomparable in his power of recognizing the former unity of what has been separated, but he has introduced no methodic art which could be learned from him in turn. Indeed, his weak point lies on just this methodic side, as has been shown above.

BOPP's greatness consists in something else, something which is independent of learning and method, namely, in what we call genius. His Comparative Grammar is based upon a series of discoveries which were not due to learning and experience, but to a gift of nature which we cannot analyze. Of 'course I do not mean to say that BOPP was not greatly indebted to his learning and his logical mind, but simply that a happy intuition plays a much more important part with him than with other distinguished philologists, as for instance with AUGUST SCHLEICHER.

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## CHAPTER II.

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### BOPP'S CONTEMPORARIES AND SUCCESSORS DOWN TO AUGUST SCHLEICHER.

BOPP was independent, but not solitary in his department. At the same period WILHELM VON HUMBOLDT, AUGUST WILHELM VON SCHLEGEL and JACOB GRIMM were working in

closely adjoining fields. I will try to estimate the influence which these men exerted on the science of which BOPP was the founder.

Of WILHELM VON HUMBOLDT, BOPP never speaks without an expression of reverence. It will suffice to quote the words with which he closes the preface to the second part of the *Comparative Grammar*:

"As to this idea [regarding the declension of adjectives], which has already been touched upon elsewhere, I have had the happiness of learning the favorable judgment, above all precious to me, of my lamented patron W. v. HUMBOLDT, in whom philology has recently lost its fairest ornament. While still overwhelmed by grief at this severe loss, I cannot refrain from here paying the tribute of most heartfelt reverence and admiration to the renowned memory of this great man, since I have been deeply impressed by his brilliant writings in the field of philosophical and historical linguistic research, as well as by the instructive and delightful intercourse I had with him, both in person and by letter."

Yet I cannot discover that W. v. HUMBOLDT exerted any considerable influence upon BOPP. HUMBOLDT's many-sided nature, with its capacity for uniting and reconciling the most endless variety of conceptions and aspirations, was not adapted to change the current of a mind of such great and simple power as BOPP's. There is nothing more difficult than to clearly define in what the influence consists which HUMBOLDT exerted upon *Indo-European* linguistic research. It is not easy within this domain to point to a field where he was pioneer, to definitely name a theory which he established, to mention a mode of view which can be wholly traced to him; yet not only BOPP, but also other representatives of the science, as POTT, SCHLEICHER and CURTIUS, acknowledge themselves HUMBOLDT's grateful pupils. To the question, how HUMBOLDT influenced these men, I think we must answer: chiefly through the totality of his own being. His lofty and disinterested love of truth; his endeavor not to lose the whole from sight while considering details, nor details while considering the whole, and thus to avoid the dangers of specialism as well as those of the previous universal grammar; the just balance of his judg-



ment; his broad mental culture, and his noble humanity, — all these qualities have a strengthening and elevating effect upon any other scholar who approaches WILHELM VON HUMBOLDT, and this sort of influence I think HUMBOLDT will still retain for a long time to come, and will continue to exert even upon those who can make nothing of his theories.

Posterity has taken a less friendly position in relation to AUGUST WILHELM VON SCHLEGEL than toward WILHELM VON HUMBOLDT. I think it is not sufficiently known outside of philological circles that the translator of Shakespeare was also the founder of Sanskrit philology. A. W. v. SCHLEGEL was in his forty-eighth year when he began to occupy himself with Sanskrit, but his admirable industry, and a gift he had of familiarizing himself with new subjects, which had been strengthened by practice in many directions, made him in a short time master of the vast difficulties which then stood in the way of the study of Indian literature. With admiration we see how rightly he at once defined the tasks which were to be accomplished :

"If the study of Indian literature is to thrive", he says in the *Indische Bibliothek*, 1, page 22, "the principles of classical philology must be applied to it, and that, too, with the most scientific acuteness. It is in vain to object that the learned Brahmans possess the knowledge of their old books through unbroken transmission; that for them Sanskrit is still a living language, and that accordingly we should go to school to them alone. With the Greeks the case was the same before the destruction of Constantinople; the knowledge of a Laskaris, of a Demetrius Chalkondylas, in regard to the ancient literature of their race, was certainly of value; yet the scholars of the West did well not to confine themselves to it. However, the acquaintance with Latin literature, which had never wholly died out, gave a tolerable preparation in Europe for the reading of Greek. Here, on the contrary, we come into a completely new circle of ideas. We must learn to understand the written monuments of India both as Brahmans and as European critics. The Homeric questions of today were not more foreign to those learned Greeks than the investigations regarding the origin of the Indian religion and legislation, the

gradual development of mythology, its unity and its contradictions, its cosmogonic, physical or historical significance, or finally, regarding the intermixtures of subsequent fraud, would be to the scholars of India. The same tasks belong to the editor of Indian books as to the classical philologist, viz.: proof of the genuineness or spuriousness of entire works and single passages; comparison of manuscripts, choice of readings, and sometimes conjectural criticism; and finally, the employment of all the artifices of the most sharp-sighted hermeneutics", etc.

A. W. v. SCHLEGEL let deeds follow in the wake of his article. His editions, according to the opinion of competent judges, accomplished all that was possible at that time, and formed the beginning of Indian philology.

A. W. v. SCHLEGEL's position toward BOPP was at first a friendly one. It was he who first (in the *Heidelberger Jahrbücher*, Sept., 1815, No. 56) announced to the public what it had to expect from BOPP; he reviewed BOPP's edition of the *Nala* with appreciation and good will, and declared in 1827, in the first letter to Heeren (*Indische Bibliothek*, 2, page 385), that BOPP and he since their acquaintance, begun in Paris in 1812, "had always worked for the same aim in friendly emulation and harmony". Subsequently the relation was changed, and in place of the "friendly emulation" grew up one of those literary enmities which were a vital necessity to A. W. v. SCHLEGEL.

A thorough polemical discussion never took place between SCHLEGEL and BOPP; there were only single sharp epigrams by A. W. v. SCHLEGEL, which were answered by BOPP. The difference was in relation to two fields, Sanskrit philology and linguistics. BOPP found time amid his vast comparative labors to gather the necessary materials for the study of Sanskrit, and to bring out the edition of *Nala*, a glossary, and above all, his Sanskrit Grammar in several different forms. And in this latter he was guilty of an omission which A. W. v. SCHLEGEL could not pardon him. BOPP never made a special study of the native Indian grammarians, but what he could use from them he took at second hand, i. e., from the grammars of his English predecessors, contenting himself with penetrating into

the sacred language of India by direct observation and comparative analysis. Now there is certainly no doubt that SCHLEGEL was quite right in theory, when he demanded that the native masters of Indian grammar should not be neglected; but it is also just as certain that BOPP was guided by a correct feeling. It would have cost him years to familiarize himself with the Indian grammarians, with the aids then at his command, and BENFEY justly remarks (*Geschichte der Sprachwissenschaft*, page 389) that it is questionable whether this eminently philological task would have been exactly adapted to BOPP.

In the other field, that of comparative philology, A. W. v. SCHLEGEL felt himself called upon as it were to defend the honor of the family. The brother took it very ill that BOPP separated himself more and more from the theory of FRIEDRICH SCHLEGEL. He regarded himself as the natural defender of the "organic" view, upon which BOPP's "agglutination theory" was gaining ground in so threatening a manner. SCHLEGEL unfortunately did not get any farther than the *announcement* of a great philological work, which was to bear the title: "Etymologicum novum sive synopsis linguarum, qua exponitur parallelismus linguae Brachmanum sacrae cum lingua Graeca et Latina; cum reliquiis linguae Etruscae, Oscae ceterarumque indigenarum veteris Italiae dialectorum; denique cum diversis populorum Teutonicorum linguis, Gothica, Saxonica, Francica, Alemannica, Scandica, Belgica." However, there exists a comprehensive and detailed critique of BOPP's grammatical works by SCHLEGEL's confidential pupil CHRISTIAN LASSEN, from which we can form some idea of how BOPP was judged in SCHLEGEL's circle. The tone in which LASSEN writes is that of the cold but just judge. What is praiseworthy is appropriately emphasized, what is mistaken is censured seriously, and only on the mention of the agglutination theory does animosity appear. The passage in question is as follows (*Indische Bibl.*, 3, page 78):

"I had intended to speak against the agglutination theory, which again recurs in this connection, but since I know that Herr von SCHLEGEL intends to discuss this point, I will gladly impose a voluntary silence upon myself in regard to the matter,

which well deserves to be treated by his superior hand. I will therefore simply mention that according to Herr BOPP's view the characteristic letters of the personal endings are really affixed pronouns, and that the origin of many tenses is sought for in the incorporated substantive verb (*as*). This word plays in general, in the book in question, the rôle of the old 'everywhere-and-nowhere', and transforms itself in Protean fashion into the most diverse forms. Although the preparations in which Herr BOPP dishes up this small word *as* seldom appear to me particularly tasteful, yet out of gratitude for his former meritorious efforts, I will call his attention to an unknown form of this verb, concerning which I am rather at a loss, — without meaning to assert thereby that it could not be employed by others for the most unexpected derivations. This form is *ās* (for *āst*), the third person singular of the imperfect active (Pāṇini, VII, 3, 97). The shortness of the form renders it very convenient for derivations, just as for word-comparison no words are so useful as the short Chinese ones, because it is only necessary to leave a vowel out of account, and to change one consonant into another, in order to manufacture Finnish, Koptic and Iroquois at will. But we reach the culmination of the agglutination theory in the derivation of the simple augment from *alpha privative*. Of all the extraordinary qualities which have been ascribed to the primeval race, this logic is the most remarkable, namely, that they said 'I do not see', instead of 'I saw'! As applied to pedagogy, this *modus operandi* would have to be expressed as follows: 'Begin the education of your children by cutting off their heads'. A verb is first deprived of its meaning, in order to construct a new form from it."

This critique of LASSEN excited great indignation among BOPP's friends, but it had no permanent influence, because it was devoid of positive statements, such as could have replaced BOPP's agglutination theory. Nor was this lack openly supplied at any subsequent time, either by A. W. v. SCHLEGEL or any of his adherents. Thus SCHLEGEL's opposition was gradually forgotten, and BOPP's theories maintained undisputed possession of the field. Not until the appearance of WESTPHAL's grammatical works did SCHLEGEL's view expe-

rience a sort of *renaissance*. Of these we shall have to speak later.

Hence we see that SCHLEGEL's influence upon comparative philology could hardly be called a directly promotive one. But indirectly it has been not inconsiderable. Since SCHLEGEL gave a powerful impulse to Sanskrit study, a portion of the gratitude is due to him which comparative research owes to Sanskrit philology.

Powerful and direct, however, was the influence of JAKOB GRIMM. JAKOB GRIMM stands wholly independent beside BOPP. When the first volume of the German Grammar came out, in 1819, BOPP had only published his *Conjugationssystem*, and a critique of FORSTER's Sanskrit Grammar in the *Heidelberger Jahrbücher*. Both of these were quoted and utilized by GRIMM, but the whole frame-work of his Grammar dates back to the pre-Boppian period. We learn from GRIMM himself in what his epoch-making achievement consists :

"I have been seized with a strong impulse", he says in the preface to the first edition of his Grammar, "to undertake a historical grammar of the German language, even if, as a first attempt, it should soon be surpassed by future writings. During my careful reading of Old German sources, I daily discovered forms and perfections which we are accustomed to ascribe with envy to the Greeks and Romans, when we contemplate the constitution of our present speech ; traces which had here remained in ruins, as if turned to stone, became gradually plain to me, and the phonetic changes were explained when the new took its place beside the intermediate, and the intermediate joined hands with the old. But at the same time there appeared the most surprising resemblances between all the sister dialects, as well as relations, hitherto overlooked, between their differences. It seemed of great importance to establish and illustrate this progressive, continuous connection down to the smallest detail ; I have thought out the execution of this plan so completely that what I am at present able to accomplish falls far short of it."

The opinion of competent judges has long since, in connection with the above words, summed up GRIMM's especial merits in the sentence : GRIMM is the creator of historical

grammar. The German Grammar had a powerful influence upon his contemporaries. In the first place, a deep impression was made by the indescribable richness of material, in comparison with which the school-boy rules of Greek and Latin grammar appear paltry. It is GRIMM's Grammar which first teaches us that complete induction is necessary to the establishment of a law. His method increased the esteem for what can be called the "natural condition" [*Naturzustand*] of language, securing to the so-called "dialects" their proper position beside the written language, not simply in the field of German, but also in that of other languages, as we can see from the words of AHRENS, who in the dedication of his work on the Greek dialects gratefully mentions the man "qui conspicuo Grammaticae Diutiscae exemplo docuit, dialectorum secundum aetates vel stirpes diversarum diligenti et sagaci comparatione quam possit in secreta linguarum penetrari".

Of especial influence upon philologists was the so-called "Law of Permutation of Consonants" [*Lautverschiebung*], which goes by GRIMM's name, although already proclaimed by RASK in its main features. While BOPP's researches chiefly aimed at the comparison and explanation of forms, so that in his system the importance of phonetic observations was not emphasized, RASK and GRIMM, by means of the law of *Lautverschiebung*, established the fact that the changes of sounds, or, as it was then expressed, of *letters*, into one another take place in accordance with laws, and above all, that a fixed historical relation can be observed between the sounds of the German on the one side and of the classical languages on the other. How influential the discovery of the law of *Lautverschiebung* grew to be we are taught by A. F. POTT, the creator of the phonetics of the Indo-European languages:

"It is by no means the least among the excellent services GRIMM has rendered to special and general linguistics, that he reinstated the letters in their natural rights, which had hitherto been curtailed by linguistic science, and raised them to the same plane on which they stand in language itself. GRIMM's historical exposition of the phonetic changes in the Germanic languages has alone more value than many a philosophical

system of philology full of one-sided and empty abstractions; for in it is demonstrated with sufficient clearness that the letter, as the palpable linguistic element, which although it is not constant, yet moves in a comparatively quiet path, is a more certain [!] clew in the dark labyrinth of etymology than the meaning of words, which is often subject to bold transformations; in it is also taught that philology, especially comparative philology, has no firm foundation when it lacks an accurate historical knowledge of the letters; finally, it shows with astounding clearness that even in the case of the simple letters no arbitrary lawlessness reigns (as, indeed, is never the case anywhere in language, the idea being only a dream of idle ignorance), but a reasonable freedom, i. e. limitation through special laws founded on the nature of the sounds themselves." (*Etymologische Forschungen*, 1, page XII.)

Perhaps the opinion is not without foundation that beside BOPP no one has exerted such an influence on comparative philology as JAKOB GRIMM (although he was never a comparative philologist in the sense that BOPP was, and did not always derive the benefit from BOPP's works which they might have afforded him); at all events, we can assert that he made the worthiest returns for the priceless gifts which accrued to German grammar from BOPP.

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The immense importance of the investigations begun by BOPP and GRIMM could not remain concealed from their contemporaries, for in truth we can just as well (as CORSEN later expressed it) refuse recognition to the sunlight as to the chief results of comparative philology. But the consequences, that is, as far as the metamorphosis of classical study was concerned, were accepted but slowly. Eminent investigators like BUTTMANN went on cultivating their land without looking over the hedge of the neighbor who had discovered a new and better method of husbandry; and pedagogues who felt themselves called upon to defend the existing order of things complained of the youths who presumed to metamorphose everything which had hitherto been held as true, but from whose labors

nothing resulted for Greek and Latin grammar but the "everlasting locative". *Allgemeine Schulzeitung*, July, 1833. All these scholars, who remained conservative either from love of ease or from prejudice, found it difficult to resist the vigorous attacks of the man who is universally recognized as the most prominent of BOPP's successors, AUGUST FRIEDRICH PORT, whose great work: *Etymologische Forschungen auf dem Gebiete der Indogermanischen Sprachen mit besonderem Bezug auf die Lautumwandlung im Sanskrit, Griechischen, Lateinischen, Litauischen und Gothischen* Lemgo 1833—1836 was the foundation of scientific phonetics.

PORT recognized that after the works of BOPP and GRIMM a sure key to etymology must now be found in phonetics (v. the interesting passage in *Etym. Forsch.*, 2, page 349), and competent judges have pronounced PORT especially fitted by nature for the accomplishment of this task. — so far as it is possible to speak of the "accomplishment" of tasks which are in their very nature endless. He showed himself, as RENAN expressed it, "un esprit à la fois sévère et hardi", as richly endowed with constructive fancy as with controlling judgment. To him are due not only a very large number of the etymologies which are held to be correct, but also the first comparative phonetic tables which embrace the whole extent of the compared languages. In my opinion it will be the verdict of the future that PORT occasionally, misled by his fancy, took the liberty of making unwarranted assumptions, as for instance in regard to the analysis of roots, a point in which CURTIUS has victoriously opposed him; but that on the whole, he more than any other man has contributed to the establishment of fixed laws for phonetic changes, and that in consideration of this fact PORT's Etymological Investigations must be counted among the fundamental works of comparative grammar, which are entitled to the next place after those of BOPP and GRIMM. In regard to the origin of inflectional forms, PORT follows BOPP, expressing the opinion that BOPP has made the subject of inflection so transparent and clear, that, with the exception of some unsolved minor difficulties, we can sufficiently comprehend its nature and character from an etymological point of view. He, as well as BOPP, considers the principle of com-



position the chief agent in inflection, although without wholly rejecting the symbolical explanation.

"Language - designation [*Sprachbezeichnung*]", he remarks, "is either symbolical or kyriological. In declension the variation according to gender [*Motion*], and the designation of gender are often symbolical; on the contrary, the mode of expressing case and number is mostly kyriological." (2, page 621.)

The inflectional endings of the verb he regards in the main as BOPP does, yet it is worthy of mention that he does not favor the latter's symbolical explanation of the *n* of the third person plural in *anti*, but considers it as a pronominal stem (as SCHLEICHER also did later), and that he declared the first person plural *masi* was derived from "I" and "thou". (2, page 710.) He is therefore as decidedly an adherent of the explanation by agglutination as BOPP, even although, as we shall see later, he was inclined to reject the historico-linguistic consequences of BOPP's theory.

Among POTT's contemporaries and successors THEODOR BENFEY must be particularly mentioned, who, on the whole an adherent of BOPP, in the very first years of his appearance before the public displayed an independent activity in several different directions. His Greek root-lexicon (*Wurzellexicon*, Berlin, 1839), the forerunner of a Greek grammar which was planned on a grand scale but not subsequently carried out, exhibited not only an astonishing copiousness of contents, but also the richest power of combination; yet it cannot be regarded as an advance upon BOPP's stand-point in its conception of phonetic changes. BENFEY's theory concerning primary verbs, with which he would fain replace what is usually called "root", and concerning the derivation of stem-forming suffixes, will be further discussed below. Here we will only mention the great honor he gained by his works in the field of Indian philology, especially by his edition of the *Sāma Veda*, Leipzig, 1848. His glossary to the *Sāma Veda* was the first work which supplied philologists with reliable material from the Vedic language for convenient use, and has exerted the most salutary influence upon etymological study.

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This reference to a book which appeared in 1848 brings us to the next period, which must be discussed in a different manner from that which has hitherto been admissible. For in the era which must now be mentioned, if not described in detail, a great number of scholars appear, whose achievements are so mutually involved that it will be wiser to shape our discussion no longer according to persons, but according to the tendencies and aims which now manifest themselves. SCHLEICHER alone, who unites a number of these efforts and brings them to a certain conclusion, will claim an individual treatment.

In the period between POTT's *Etymologische Forschungen* and SCHLEICHER's *Compendium* a very considerable extension of our knowledge took place, and to this fact we must first turn our attention. Perhaps no extension of knowledge was ever fraught with higher results to philology than that which occurred in the Indian field. Our acquaintance with Indian literature began with the Indian middle ages, and not till afterwards, when (from about the year 1840) Vedic studies began to flourish, did Indian antiquity become known to us. Through the works of ROSEN, ROTH, BENFEY, WESTERGAARD, MÜLLER, KUHN, AUFRECHT and others, a mass of new and reliable material was in a comparatively short time brought before the etymologists, who had hitherto been rather scantily provided with Indian lexicographic aids. WILSON's *Lexicon* (concerning which, v. the preface to the first volume of BÖHTLINGK & ROTH's dictionary, as well as the article of SCHLEGEL in the *Indische Bibliothek*, 1, page 295 seq.) was far from being a historically arranged lexicon, and the Indian lists of roots are an aid which is attended by peculiar dangers. Even if we could assume that the lists which the Indian grammarians drew up were made and transmitted with perfect accuracy, they could be used only with caution for etymological comparisons, for the manner in which the Indian scholars denote the meanings is different from that to which we are accustomed. When they append to a root the locative of a substantive, to determine its meaning, they do not always intend to denote the individual sense, but often only the general category of meaning under which the verb falls. On this account

the critical editor of these lists (WESTERGAARD, *Radices linguae sanscritae*, Bonn, 1841) uttered the following warning against a too confident use of them :

"Ceterum puto cavendum esse, ne illa grammaticorum de potestate radicum decreta nimis urgeantur, nam illis nihil vagius, nihil magis dubium et ambiguum esse potest; sic, ut unum modo exemplum afferam, vocula quae *gatau* est, unumquemque motum ut eundi, currendi, volandi etc. indicat, quin etiam exprimit mutationem, quam subit lac coagulando, et nescio quam multas alias."

But the assumption made above will not hold good. Naturally all the roots are not correctly given. If, therefore, we would proceed prudently, we cannot feel confidence in a root until we have authentic proof of its occurrence in the literature (which we do not possess in the case of many), unless some reason can be found why a root would naturally be wanting in the written language, as for instance is the case with *pard* = *πέρδομαι*. But beside this, they are not correctly transmitted, having been exposed to all the injuries which time is wont to cause to literary products. And this corruption has not only affected the roots (WESTERGAARD, page IX, mentions no less than 130 roots figuring erroneously with his predecessors, part of which had been employed for comparisons), but also the meanings given. We can see how much opportunity there was for error, and as a matter of fact, there has been much harm done through etymological use of unauthenticated roots and erroneously assumed meanings. That this source of error is now closed, is due to the labors of the above-mentioned men, first of all, however, to the Sanskrit lexicon of BÖHTLINGK and ROTH, that incomparable master-work, which was almost as epoch-making for linguistic science in general as for Sanskrit philology.

Beside the Sanskrit, the Slavo-Lithuanian and Celtic languages (the latter of which was assumed even by POTT in his *Etym. Forsch.*, 2, page 478, to belong to another family than the Indo-European, but to have been mingled with the latter in pre-historic times) were the object of attention and study. Yet we can say that at the period which now occupies

us, the Sanskrit, the classic and the Germanic languages always took the lead.

Not only the extension of knowledge, but also the position taken with regard to phonetic laws, seems characteristic of this period. What I mean is clearly illustrated by a passage from CURTIUS' remarks on the extent of phonetic laws (*Ueber die Tragweite der Lautgesetze, Berichte der phil.-histor. Classe der Königl. Sächs. Gesellschaft der Wissenschaften*, 1870), which is as follows :

"Since the first bold onset of the founders of our science, a younger generation, from 1840 or thereabouts, has had for its watch-word: 'strictest regard for phonetic laws'. The abuses of which even meritorious scholars had been guilty, through the assumption of weakenings, corruptions, mutilations etc., had engendered a well-founded distrust, which inevitably led to a greater strictness and conservatism in this respect. The results of this tendency, which in this sense is a more rigorous one, we can certainly call beneficent. More accurate observation of the phonetic changes and their causes, more careful separation of the individual languages, linguistic periods and linguistic varieties, more definite insight into the origin of many sounds and sound-groups have been attained. In this respect we can see much farther and more clearly than twenty years ago, as is most evident from the fact that many an airy assertion formerly propounded has been recognized as impossible even by its originators."

Finally, we must regard as especially important the attempt to separate the individual languages more strictly from each other. BOPP did not scruple to confirm an asserted phonetic change in Latin by a reference to the Armenian. Such freedom could from this time forth no longer be tolerated. Each separate language must be recognized in its own peculiarities. In this direction the works of GEORG CURTIUS (of whose labors in the general Indo-European field we shall speak later) were of great influence, i. e., his investigations concerning the formation of moods and tenses in Greek and Latin, and his *Grundzüge der griechischen Etymologie*. It was the aim of this latter book to record the sure gain accruing to Greek etymology from linguistic comparison, and this task

has been, to use ASCOLI's words, executed with that masterly power in the use of positive, creative criticism which characterizes the author. Less fortunate were CORSEN's efforts in the Italic field. BENFEY (*Orient & Occident*, 1, page 230 seq.) has justly censured the individualizing style of this scholar, whose method of observation must necessarily cause that to be regarded as individually Italic which certainly had belonged to the Indo-European parent speech. Yet it is impossible to deny that CORSEN, especially in the first edition of his work, where comparison is not so prominent a feature, contributed to the better knowledge of the Italic languages in a way to deserve very considerable credit. (Cf. on this point ASCOLI's admirable verdict in the *Kritische Studien*, page IX.)

Many of the attempts of this period (not all, for BENFEY's school has always gone its own way) are, to a certain extent, summed up in SCHLEICHER's *Compendium*. It therefore seems to me expedient just here to devote to SCHLEICHER a somewhat more detailed consideration.

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## CHAPTER III.

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### AUGUST SCHLEICHER.

On our first acquaintance with the works of AUGUST SCHLEICHER (born 1821, died 1868) we are compelled to observe that an influence, recognized by himself, was exerted upon this scholar from two fields of science which lie outside the domain of philology, viz., HEGEL's philosophy, of which he was an adherent in his youth, and modern natural science, for which in the latter part of his life he showed a passionate predilection<sup>1</sup>). Let us try to define the nature and strength

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<sup>1</sup>) Although the inclination appeared much earlier, — cf. *Formenlehre der kirchenslawischen Sprache*, preface, page VI, note.

of this influence in general, before we follow SCHLEICHER into the details of his investigations.

At the outset, in the introduction to his first great work, the *Sprachvergleichende Untersuchungen* (Bonn, 1848), SCHLEICHER shows himself an adherent of HEGEL, as we can see from the ideas he introduces there.

Language (as he explains in detail) is made up of meaning and relation. The former is contained in the root, the latter in the formative syllables. Therefore there can be three and only three classes of languages. Either the meaning alone is designated, as occurs in the isolating languages; or the sound showing the relation [*Beziehungslaut*] is affixed to the sound showing the meaning [*Bedeutungslaut*], as happens in the agglutinating languages; or, finally, the two varieties of sounds form the closest union, as in the inflectional languages. A fourth case is not possible, since the *Beziehungslaut* cannot stand alone. Now three periods of development must correspond to these three norms of the system. We are accordingly compelled to assume that the isolating languages represent the oldest form, that from these have arisen the agglutinating, and from these in turn the inflectional languages, so that the last stage of the process contains the two previous ones. But SCHLEICHER argues further that our actual experience is not in harmony with this theoretical construction, for we find the languages which come within the circle of our experience not in a state of development, but of decomposition; higher forms do not arise before our eyes, but existing ones fall to pieces. Yet since philosophical construction and the result of observation must both claim credence, the inevitable conclusion is that the two processes in question must be located in different periods. Languages were formed in pre-historic times, and are destroyed in historic ones. The making of languages and of history are activities of the human mind which mutually exclude each other.

The above is a condensed reproduction of SCHLEICHER's reasoning, which recurs, at least partially, in his later works, and was not wholly set aside even by the leaning to natural science which was so strong in his latter years.

This is not the place to criticise these views, whose

weakness is self-evident at the present day, but it may be interesting to observe in how far SCHLEICHER shows himself dependent upon HEGEL. This dependence is evidently slight as regards material. In the first place, the division of language into the three groups mentioned above was not derived from HEGEL, but from experience. SCHLEICHER had worked it out for himself under the guidance of FRIEDRICH SCHLEGEL and WILHELM V. HUMBOLDT. (Cf. the *Beiträge* of KUHN and SCHLEICHER, 1, page 3, note.) Further, the opinion that inflection sprang from composition followed from BOPP's form-analysis, which SCHLEICHER on the whole favored; and BOPP had likewise originated the theory that it is only possible to observe languages (at least the Indo-European) while in a state of decline. We can therefore recognize the material influence of HEGEL only when SCHLEICHER assumes that in the development of mankind we must distinguish between a pre-historic period, during which the mind was still in dreamy thralldom, and a historical period, in which it awakes to freedom. This classification of human development into a pre-historic and a historical period (language being perfected in the pre-historic period) was always retained by SCHLEICHER, and it is not improbable that this view was induced by HEGEL.

While, therefore, but little of the subject-matter in SCHLEICHER's writings could be pronounced Hegelian, yet in the early work which was mentioned above, the influence of HEGEL is unmistakable in the formulation of the thoughts and the structure of the argument. This influence waned with SCHLEICHER's growing maturity, yet we can still feel it in his later works, and trace it here and there, particularly in his terminology.

We accordingly come to the conclusion that the influence of HEGEL's philosophy on SCHLEICHER was only a moderate and comparatively superficial one.

SCHLEICHER took a different position from most philologists in regard to the natural sciences, inasmuch as he really possessed considerable knowledge of them. He was especially versed in botany. According to scientists who knew him, he was celebrated for his admirable preparations for the microscope, as well as for certain productions of horticultural art. As the years went on, these studies and favorite pursuits gained

an ever greater influence upon his philological views. When he walked up and down in his beloved garden, and analyzed forms of speech, the thought must have often occurred to him that the analyzer of word-forms and the analyzer of plants have in reality the same profession; and when he contemplated the law-abiding nature of linguistic development, which it was his most earnest endeavour to demonstrate, the idea seemed to him very natural that language was nothing less than an organic being. In his methodical mind these thoughts and impressions took the shape of a serious system, whose axioms are as follows:

Language is a natural organism; it lives like other organisms, although its mode of action is not that of man. The science of this organism belongs to the natural sciences, and the method by which it must be treated is that of natural science.

SCHLEICHER set great value upon these axioms, and I would venture to assert that if he had been asked in his last years what in his own opinion constituted his chief service to science, he would have answered, the application of the method of natural science to philology. The judgment of the majority of his contemporaries was different, and at the present day it is almost unanimously agreed that SCHLEICHER's three axioms cannot find approval. BOPP had already applied the expression "organism" to language, but he had simply meant that language is not arbitrarily manufactured. Such a figure can be tolerated, but when the metaphor is taken literally, the contradiction becomes evident. Language is not a being, but the utterance of beings; accordingly, if we are to use the phraseology of natural science, it is not an organism, but a function. It will also be found extremely difficult to classify philology with the natural sciences. Since language is manifested in human society, the science of language cannot belong to the natural sciences, at least, if this name is used in the accepted technical sense. And finally, as regards the method, I feel certain that there is no single method adapted to all natural sciences. For one part of the natural sciences the application of mathematics is characteristic; for another, experiment; for a third, to which biology belongs,



the so-called "genetic" method. And to this last the philological method certainly bears some resemblance, inasmuch as in both sciences we strive to understand the historical development of their objects.

However, it is not my intention to discuss these views more in detail. There is no necessity, for my immediate purpose, of showing whether SCHLEICHER's views are right or wrong, but only how they arose and worked within him.

It cannot be denied that SCHLEICHER's fondness for natural science is more plainly traceable in his chief works than Hegelianism. But we can only judge from detailed observation how deep these influences were in separate instances. Accordingly I now pass to a critical survey of SCHLEICHER's philological achievements and views.

In SCHLEICHER's first works we can still plainly discern the philosophical atmosphere in which they originated, inasmuch as they aim less at a through investigation of details than a systematic survey of a broad field. For in the first part of his *Sprachvergleichende Untersuchungen* he traces certain influences of *j* (the so-called "zetacism") through as many languages as possible, and in the second part [*Die Sprachen Europa's*] he gives the outline of a system of linguistics. Very similar in character is a much later work, *Die Unterscheidung von Nomen und Verbum in der lautlichen Form* (*Sächs. Ges. d. Wiss.*, Leipzig, 1865). In addition to these general studies SCHLEICHER began very early to appropriate to himself a special field, the Slavonic languages, and here he has acquired a reputation of which no change in time or opinion can rob him. SCHLEICHER stands beside Miklosich in this field somewhat as BOPP does beside GRIMM in the Germanic one. He more than any one else has helped to illumine the Slavonic languages by the light of comparison. In his Lithuanian studies he brought a wholly new material within the reach of science, by collecting the Lithuanian forms here and there, as a botanist does his specimens, and preserving them for all time in the herbarium of his grammar. In consequence of his academical duties (in Bonn, Prague and Jena) he was also compelled to devote his constant attention to the other Indo-European languages, and thus was prepared, in the broadest

imaginable way, for the chief work of his life, his *Compendium der vergleichenden Grammatik der indogermanischen Sprachen* (Weimar, 1861), which we must regard as the crowning glory of his career, since an early death called him away from still greater plans.

SCHLEICHER's *Compendium* is the conclusion of a period in the history of philology, the beginning of which was formed by BOPP's works. This is the reason why the general impression produced by the Comparative Grammar on the one hand and the *Compendium* on the other is so utterly different. BOPP was obliged to prove the essential identity of the Indo-European languages, while Schleicher regarded it as established; it was BOPP's place to acquire, SCHLEICHER's to organize. BOPP's attention was especially occupied with what was common to all Indo-European language; it was SCHLEICHER's task to make the individual languages stand out clearly on this common background. Hence the comparative Grammar is a connected narrative, while the *Compendium* could without much trouble be resolved into a number of separate grammars. The author of the Grammar in his demonstration of single points employs principally the form of investigation, which he handles with a natural charm of manner; in the *Compendium*, on the contrary, we find almost exclusively the concise and monotonous style of assertion. The older work may be compared with the exposition of an interesting lawsuit, the younger with the paragraphs of a code of laws.

The difference is less striking when we compare the views which are brought forward in the two books. In the first place, BOPP's theory of the origin of inflection was in the main adopted by SCHLEICHER, although he formulated it differently. Like BOPP he regarded roots, which in his opinion were inevitably monosyllabic, as the constituent elements of the Indo-European. Like BOPP he distinguished two classes of roots (although he considered it probable, contrary to BOPP, that the so-called "pronominal" roots were derived from the verbal ones). Like BOPP he recognized affixed pronouns in the stem and word-forming suffixes. Only on single points was his opinion different. For instance, in the explanation of the middle endings, where BOPP was uncertain, he expressed

himself decidedly in favor of the theory of composition, which he carried out to the smallest detail. He followed POTT in his view of the plural endings of the active; he held that the element of the optative was not the root  $\bar{i}$  or  $i$ , but the pronominal root  $ya$  (without, indeed, informing us how the meaning of the optative could be explained under this assumption); and in the subjunctive, which BOPP had not regarded with certainty as a special mood, he discovered a pronominal root  $a$ .

There does seem to be a marked difference in the interpretation given to the idea "inflection", which SCHLEICHER in the *Compendium*, § 2, defines as follows: "The essence of inflection lies in the vocalism". These words, which at first sight are very striking, must be understood thus. SCHLEICHER recognises two classes of languages in which the forms originate by means of composition, the agglutinating and the inflectional. He finds the peculiarity of the latter in their ability to change the root-vowel for the purpose of expressing relation; so e. g.  $\epsilon\bar{\iota}\mu$  is compounded of  $\iota$  and  $\mu$ , the  $\iota$  being changed to  $\epsilon$  in order to express the relation. The inflectional languages accordingly possess the principle of composition, and beside this the capacity of changing their root-vowel as just shown. But in his definition SCHLEICHER mentions only the latter of these distinguishing qualities.

We can readily perceive that beneath this form of the definition there lurks a remainder of SCHLEGEL's conception of inflection, with which at an earlier period SCHLEICHER's views were more in accord; but this remainder is so trifling in its actual significance that it may be overlooked, and we can justly characterize SCHLEICHER as an adherent of BOPP's agglutination theory.

SCHLEICHER also agreed with BOPP in assuming that not the primitive period alone had the capacity to produce new formations by agglutination, but that composition might also appear in the individual languages, in the same way as in the parent speech, as for instance in the Latin perfect.

The difference seems widest in the department of phonetics, but even here it is not a difference of principle. In principle SCHLEICHER's stand-point was the same as BOPP's, since it was also his opinion that the phonetic changes of language

do not exhibit development, but decline, and since he admits occasional (although much rarer) exceptions to the prevailing phonetic laws.

But there is a very considerable difference in degree between the phonetic systems of the two scholars. What a stately appearance SCHLEICHER's *Lautlehre* presents, occupying, as it does, half of the whole Compendium, compared with BOPP's scanty and unevenly written chapter, which bears the title "System of writing and phonetics" [*Schrift- und Lautsystem*]! It was SCHLEICHER's task to sift down and turn to account the great mass of detailed investigations which had been undertaken since BOPP's time by POTT, BENFEY, KUHN, CURTIUS and others. In his treatment of the subject we can observe the progress intimated above. The differences of the separate languages are taken into account, all related instances are carefully placed side by side, and the probability of each single instance measured from the result obtained. Thus SCHLEICHER established a long series of carefully weighed, well-grounded phonetic laws, which were destined to serve as a regulating principle for every etymologist, and he has undeniably won great credit by this task of sifting and arranging.

Of course all such laws have only a provisional value. For since obvious etymologies form the material from which the phonetic laws are derived, and this material can perpetually increase and change, it is always possible that new phonetic laws should be recognized, or old ones transformed. This idea, whose correctness has been amply confirmed by experience (for how much that is new has been discovered by FICK alone!) was not fully appreciated by SCHLEICHER. This was probably owing to the fact that he himself, with his methodical mind, had no conception of that combination of ideas which is indispensable to the discovery of new laws, and therefore undervalued etymologizing in general.

We conclude from the foregoing that in all important points which have hitherto been mentioned, the difference between BOPP and SCHLEICHER cannot be called a difference of principle. But one point still remains, which at all events brings SCHLEICHER's originality into the clearest light, — I

refer to the reconstructed *Indo-European parent speech* [*Ursprache*]. I find the earliest mention of this parent speech in the preface to the *Formenlehre der kirchenslawischen Sprache*, where we read:

"In comparing the linguistic forms of two kindred languages, I try first of all to trace back both the compared forms to their probable fundamental form, i. e., the shape which they ought to have, leaving out of account the later changes; or at any rate, to bring them upon the same phonetic plane. Now since the oldest languages of our family (even the Sanskrit) do not exhibit their oldest phonetic form, and since the different languages are known to us in very different degrees of age, this difference in age must first be eliminated as far as possible, before there can be any comparison; the given quantities must be reduced to common terms before we can compare them, whether the expression thus obtained be the oldest form which can be deduced for both the compared languages, or the oldest form of one of them."

Hence, in comparing two languages, we can either reduce the form of one language to that of the other (e. g., Slavonic *pekąšta* to a Sanskrit *\*pacantyaśya*, — v. the work quoted above), or trace both forms back to a common primitive form. The first method, so far as I can see, has very seldom been actually applied by SCHLEICHER; the second, on the other hand, if for "the comparison of two languages" we substitute the words "comparison of all Indo-European languages", contains the following rule for the construction of fundamental Indo-European forms: from a form which appears in all languages, subtract all that is due to the special development of the individual languages, and what remains will be the primitive form. An example will make these directions clear. "Field" in Sanskrit is *áyas*, in Greek *áγρός*, in Latin *ager*, in Gothic *agr*. We know that in Gothic *g* has become *k*, and that an *a* was lost before the *s*; thus we obtain from the Gothic the primitive form *agras*. We know further that the Greek *o* is derived from *a*, so that we likewise obtain *agras*, and so with each language in turn. Hence *agras* may be regarded as the primitive form, and by a similar process we deduce the accusative *agram*, genitive *agrasya*, ablative *agrāt*,

nominative plural *agrāsas* etc., as well as a large number of pronouns, prepositions etc. All these forms together make up the Indo-European parent speech; or, expressed in historical style: the parent speech is the language which was spoken immediately before the first separation of the primitive Indo-European race.

SCHLEICHER did not always content himself with this simple and clear notion of the parent speech, for he often ascribes to it a quality which cannot be derived from the previous definition, — the quality of complete integrity of its original structure. An example will best explain what is meant. The nominative of the word for "mother" is in Sanskrit *mātā*, in Greek *μήτηρ*, in Lithuanian *motė*, in Old Slavonic *mati*, in Old High German *muoter*. Nowhere does an *s* appear in the nominative. Accordingly, by comparing the separate forms we can only obtain the form *mātār* or *mātā* (the latter if we assume that the *r*, as for example in *μήτηρ*, was in the individual languages introduced into the nominative from the oblique cases), but not the form *mātars*, as SCHLEICHER does. He assumed this form because *mātār* is the stem, and *s* the suffix of the nominative, and he felt convinced that in the parent speech so-called "phonetic laws", mutual influences of sounds, and similar phenomena, were not yet in existence. But this supposition is quite arbitrary, for if the primitive speech was spoken by human beings, it must have shared the fate of all language, viz., change in phonetic and morphological constitution. There is, then, no reason why we should not ascribe to the parent speech forms like *mātār* or *mātā*. It is true that in a still older period the form may have been *mātars*, as SCHLEICHER assumes, but then it would be necessary to distinguish the different periods of the primitive language, so that we should not put older and younger forms upon the same plane, as SCHLEICHER seems to have done. The failure to make this distinction has undeniably introduced a certain ambiguity into SCHLEICHER's notion of the parent speech. In the following discussion I venture to leave this difficulty out of account, and will understand the term "parent speech" only in the sense previously defined, i. e. in the sense originally intended by SCHLEICHER.

Is it, now, SCHLEICHER's opinion that a historical reality must be ascribed to the forms of the parent speech, taken in this sense? I think the reader of the Compendium will be inclined to answer this question in the affirmative, and will be somewhat surprised to find in the appendices (*Chrestomathie*, page 342) the following remark: "The assumption of these primitive forms does not necessarily imply the assertion that they ever possessed an actual existence." In order to explain this apparent contradiction I will choose the form of independent discussion, proceeding in the following order: I will first formulate the objections which have been brought against SCHLEICHER's parent speech (in the sense described above), and then try to determine their true value.

The first difficulty is naturally due to the demand that in the case of a certain form each individual language shall be consulted. This demand, however, can be satisfied only in the rarest instances, for how few are the words and forms which we can trace through all the languages! But in actual practice this objection has little weight. For we must consider that it is possible to point out in all languages quite a number of inflectional suffixes, or at least traces of them; and since we know the phonetic laws which would come in play, we can say in the case of a good many word-stems what their form must have been in a given individual language.

A second objection is of a more serious nature. Is it really possible to define the point where the development of each separate language began? Can we determine with certainty whether a certain modification of sound or form belonged to the primitive speech or originated in the individual language? SCHLEICHER had definite views on this point. For instance, he thought it possible to assert that the parent speech possessed the following sounds :

## Consonants :

<i>k</i>	<i>g</i>	<i>gh</i>
<i>t</i>	<i>d</i>	<i>dh</i>
<i>p</i>	<i>b</i>	<i>bh</i>
<i>j</i>	<i>s</i>	<i>v</i>
<i>n</i>	<i>m</i>	<i>r</i>

## Vowels :

<i>a</i>	<i>i</i>	<i>u</i>
<i>aa</i>	<i>ai</i>	<i>au</i>
<i>āa</i>	<i>āi</i>	<i>āu</i>

How did he reach this conclusion? In separate fields the way had been prepared for him, as in the field of the *a*-vowels. It is well known that the Indo-Iranian group of the Indo-European possesses no *ē* and *ō*, but exhibits an *ā* where the other languages have these vowels. BOPP was only at first of the opinion that *ē* and *ō* originally belonged also to the Sanskrit, and were subsequently lost; he then adopted the view of GRIMM (Grammar, I, 2<sup>nd</sup> edition, page 594), who in connection with his Gothic researches denied that *ē* and *ō* were original, so that for the Indo-European there remained three simple primitive vowels, *a*, *i*, *u*. This assumption also gained favor in consequence of the high esteem which the number three is wont to enjoy; POTT, for instance, begins the section on vowels in his *Etymologische Forschungen* with the remark: "It seems to follow from historical and *physico-philosophical* grounds that language possesses but three simple fundamental vowel sounds, viz., *ā*, *i*, *ū*." Thus the hypothesis of GRIMM seemed to find confirmation in all directions, and was accepted by SCHLEICHER. He assumed that the primitive language agreed with the Sanskrit in the simplicity of its vocalism, while the more diversified Greek exhibited a condition of greater development or deterioration. For the consonants, however, an opposite conclusion was reached. The cerebrals of Sanskrit had been early regarded with suspicion, the assumption being made that the Hindus had obtained these extraordinary sounds from barbarian aborigines; the palatals also were found in many cases to be younger than the gutturals, as for instance in reduplication (*cakāra* from *kar*). In this point, therefore, the Greek appeared to have preserved the original condition of things, while the Sanskrit had deteriorated, and the main conclusion was that the rich and diversified phonetic material, which the individual languages either exhibit or must have once exhibited, arose by means of various processes of division and multiplication out of a limited and simple phonetic material in the primitive speech. From the analogy of this result SCHLEICHER drew the further conclusion that the phonetic condition at a still earlier period must have been yet more simple:

"At an earlier period in the life of the Indo-European



parent speech, the three aspirates and the three diphthongs with  $\bar{a}$  were wanting; in the original condition of language, before it had become inflectional, there were no diphthongs at all. Accordingly, the Indo-European probably possessed at first six momentary sounds [*momentane Laute*], viz., three surds and three sonants; six consonantal duratives [*Dauerlaute*], viz., three spirants and three so-called 'liquids', i. e. the two nasals  $m$ ,  $n$ , and  $r$  ( $l$  being a sub-variety of  $r$ ); and six vowels. In a later stage of the language, shortly before the first separation, there were nine momentary and nine vowel sounds. The symmetry of the numerical relations apparent in the number of the sounds must not be overlooked." (*Compendium*, § 1, note 1.)

This theory of development offers a broad field for criticism. In the first place, the general statements must be put aside as not conclusive. For the remark that in earliest times the phonetic condition must have been a very simple one, can with equal justice be confronted with a contrary assertion. We notice, indeed, that the individual languages often lose in phonetic volume. Why should it be impossible to assume that the parent speech was richer than any of its daughters? And the symmetry of numerical relation so emphasized by SCHLEICHER would only be of value if it could be shown to result from the nature of the human vocal organs, which is not the case. Therefore the special grounds in each individual instance can alone decide; and these seem to speak against the tendency of SCHLEICHER's hypotheses. On the contrary, it seems as if we must rather assert that the primitive language resembled the Greek most closely in the diversity of its vocalism, and the Sanskrit in the manifoldness of its consonantism. For instance, we can in my opinion declare with certainty that two sets of  $k$ 's existed in the parent speech, one of which was liable to palatalization; further, that the parent speech possessed an  $e$  (or  $\acute{a}$ , if that style of writing is preferred).

What is true of  $e$  must probably be assumed for  $o$  also, and the words of the primitive speech which were reconstructed by SCHLEICHER would, according to these views, present a very different aspect. SCHLEICHER once indulged in the pleasantry of writing a fable in the Indo-European parent speech, which

he entitled: *Avis akvāsas ka* ("The sheep and the horses"). According to the newer theories this title would run: *Ovis ek<sub>1</sub>vōs k<sub>2</sub>e* (the *k<sub>2</sub>* in this case designating the palatal *k* of the parent speech). "He saw" would be represented no longer by *dadarka*, but by *dedork<sub>1</sub>e*; the accusative of a participle "riding" not by *vaghantam*, but by *vegh<sub>1</sub>ontm* (where the *m* is syllabic), etc.

Ten years from now the transcription will perhaps have assumed a different coloring. It accordingly follows that "parent speech" is nothing but a formal expression for the changing views of scholars in regard to the nature and extent of the linguistic material brought by the individual languages out of the common tongue. With this definition of "parent speech" the question of the historical value of the reconstructed forms is settled. It can neither be doubted nor denied that the primitive language possessed a great number of words which were capable of grammatical inflection, and a series of uninflected words. But whether it wore exactly the aspect claimed for it by present investigation, whose stand-point is mirrored in these reconstructions, it is of course impossible to determine.

The use and significance of these forms can now be defined. They add no new material to our knowledge, but they bring more clearly before us what is already known. They have the same value for linguistic research which curves or similar illustrative aids have for statistics, and form besides a means of demonstration whose usefulness must not be undervalued. At the same time, the necessity of postulating fundamental forms impels the investigator to always inquire whether each particular form which is under consideration must be regarded as original or as a new formation, and does not suffer him to rest satisfied without a thorough mastery of all difficulties, phonetic or otherwise.

We see from the whole previous discussion that SCHLEICHER does not essentially differ from BOPP in principle. His individuality consists in his method of demonstration by means of the parent speech.

If we recognize this fact, we can at once answer the question, how far the scientific tendency of SCHLEICHER was materially influenced by natural science. Of course this question can only be asked in regard to that part of his system in which he differs from BOPP and other philologists, that is, in regard to the parent speech. And it really would seem as if he owed this to natural science. For SCHLEICHER himself introduces his parent speech with the following remark (*Formenlehre der kirchenslawischen Sprache*, page VII) :

"This procedure, like the method of linguistic science in general, agrees with the method of the natural sciences, of which linguistic science forms a part. The comparative anatomist never compares the form of the skull of two animals by taking the skull of a new-born specimen of the one sort, and the skull of an adult of the other; if the needful material is wanting, as is often the case in fossil remains, he does just what we do; according to known laws he reconstructs what is lacking, on the same plane of age with the specimen before him."

Nevertheless, I cannot believe that SCHLEICHER was inspired by the anatomist to emulate his example; it seems to me more probable that he sought among the scientists for analogies to his own procedure, after it was already complete. For on the one hand, the reduction of forms of different ages to the same plane of age seems to me so natural a thought, and one which would so easily suggest itself from the very task in question, that I should hardly suppose it would have originated in a foreign field; and on the other hand, I should be inclined, despite my slight acquaintance with anatomy, to assert that in the passage above quoted SCHLEICHER views the proceedings of the anatomist through philological spectacles.

I come to the conclusion, therefore, that in this point also nothing has been borrowed from natural science; nor has any transfer of the method of natural science taken place. Indeed, the whole idea of a transfer of methods strikes me as rather extraordinary. How can it be possible, after a method of procedure has adapted itself to the peculiarities of one object, to apply it with profit to a different one? SCHLEICHER himself

has not done this. Little as he would himself acknowledge it, he, as well as BOPP and GRIMM, POTT and CURTIUS, is, in the essence of his being, — a philologist.

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## CHAPTER IV.

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### NEW ENDEAVORS.

We cannot regard the Compendium as a summary of the collective philological labors of the period. For — to mention only what is most important — POTT displayed his peculiar and partially isolated activity in other paths than SCHLEICHER, and BENFEY and his school also formed a separate group. POTT could not become reconciled to SCHLEICHER's reconstructions, and BENFEY and his adherents held that SCHLEICHER, by his phonetic laws, wrongfully fettered the movement of sounds. SCHLEICHER himself considered that CURTIUS, and CORSSSEN stood nearest to him in the philological field. CORSSSEN's importance was at that time over-estimated (as remarked above, page 39), but we may justly regard SCHLEICHER and CURTIUS as the chief representatives of a tendency which has exerted and still exerts a powerful influence, not only on the study of the individual languages, but upon all views regarding the aim and method of linguistic science. For the labors of CURTIUS were not confined to the Greek field, but in his essay on the chronology of Indo-European linguistic research he furnished an amplified and improved sequel to that portion of the Compendium which treated of the parent speech; in this publication he made the attempt (as SCHLEICHER had not yet done) to follow out the historical development of the original Indo-European language.

After the tendency described above had for a number of years held the prominent position of interest, the objections which had been raised against it from different directions began to make themselves more strongly felt than hitherto;

fresh scruples appeared on individual points; attention was gradually withdrawn, as if wearied, from certain sections which had hitherto claimed especial notice, while other portions, which had remained in the shade, became more plainly discernible; in short, a new tendency began to gain ground, which was partly a continuation of the previous endeavors, partly an attempt to oppose, ameliorate and expand them. The impulses to this new movement did not proceed from one point, but from many, both simultaneously and successively, so that it would almost seem as if it were more correct to speak not of one tendency, but of several diverging ones. Yet I think that the common element in these new efforts is the truly important and essential thing, and with this I will endeavor to acquaint the reader in a few words.

I have shown that from the first it was the prevailing tendency of BOPP's works to explain the origin of grammatical forms. In the beginning BOPP sought this explanation in each individual language, and said, for example, that the aorist was formed in Sanskrit by composition with *as*, in Greek similarly by composition with *ἐξ*, and so on. Now the more plainly it was recognized that the explanation of the forms must be sought, not in the individual languages, but in the *common speech* [*Gesamtsprache*] from which they have all sprung, the more this latter came into the foreground; and it is therefore perfectly consistent that the primitive speech plays so important a part precisely in the works of SCHLEICHER and CURTIUS, who, like BOPP, aim at an explanation of the grammatical forms. On the other hand, it was also natural that in course of time the objections which could be brought against analyzing a form belonging to the primitive language became more strongly emphasized. The realistic age, which prefers to hold itself aloof from things which cannot be known, has become more and more conscious of the hypothetical nature of such analyses, and we can accordingly assert that among a not inconsiderable number of philologists, all *glottogonic* hypotheses, i. e. all attempts to explain the forms of the parent speech and to build up a history of inflection upon them, have come into disfavor. (Meanwhile, SCHLEICHER's parent speech as defined above can of course be defended throughout.)

In illustration of this view I will quote a few words of JOHANNES SCHMIDT, who, after emphasizing the difficulties attendant on the customary explanation of the optative (by composition with the root *i* or *yā*), expresses himself as follows (*Kuhn's Zeitschrift*, 24, page 320):

"I do not feel myself under obligation to propose a new explanation. It is the task of Indo-European linguistic science to demonstrate what the forms of the parent speech were, and by what methods those of the individual languages have sprung from them. We are in most cases as incapable of interpreting the significant value of the formative elements which are affixed to the so-called root, and for the same reason incapable, as the one-sided Greek grammar was of explaining the elements of Greek words. In this field the recognition of ignorance increases from year to year, as befits a healthy science."

Another consideration is closely connected with the above. In order to explain the forms of the primitive speech, it is customary to analyze them into the parts from which they are supposed to have arisen, e. g. *dāsyati* "he will give" into *dā-sya-ti*. Now the same process of analysis can also be employed in those languages which, like the Sanskrit and Greek, have remained upon a tolerably ancient plane of development. Thus BOPP resolves a form like *δοθησόμεθα* into *δο-θη-σο-μεθα*. Can it, now, be assumed that the affixion of these elements first took place in Greek? Certainly not. The more thoroughly the comparison of the Indo-European languages has been prosecuted, the plainer the following principle has become: inflection was completed in the parent speech, and only finished forms were transmitted to the individual languages. If this is correct (and who can doubt it?), the question at once arises: how, then, are new formations possible in the separate languages? The credit of proposing this question is due to MERGUET (*Die Entwicklung der lateinischen Formenbildung*, Berlin, 1870)<sup>1)</sup>, and that of answering it, to those scholars who have given particular prominence to the idea of forma-

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<sup>1)</sup> V. an essay by the same author, in *Neue Jahrbücher für Philologie und Pädagogik*, pages 109, 145 seq.

tion by analogy, especially WHITNEY, SCHERER and LESKIEN. (Cf. MISTELI, *Lautgesetz und Analogie*, in STEINTHAL's *Zeitschrift für Völkerpsychologie und Sprachwissenschaft*, 11, page 365 seq.). Since in a finished language it is no longer possible to construct new forms by affixion of the constituent elements, unless these elements are themselves finished words, all other new formations can only be due to analogy. New formations are imitative formations. With this view, the principle of analogy came naturally into the foreground in the explanation of forms, and many individual forms, as for instance the Latin imperfect, future etc., had to be regarded otherwise than hitherto.

Another incitement to progress appeared in the field of phonetics. After the Sanskrit, Greek and Italic languages had for a long time taken the lead, and this, too, in their earliest stage of development, the more modern idioms gradually became the object of increased attention. Now these (for instance, the Slavonic and Romanic languages) possess a wealth of phonetic development, joined with a delicate precision in phonetic distinctions, which is fairly astounding. At the same time, the study of the physiology of sounds [*Lautphysiologie*], which was again prosecuted with especial zeal, brought to light the manifold richness of their phonetic structure. What was the objection to ascribing to earlier periods the same manifoldness of sound which we observe in the languages of today? Why should it not also be assumed for the common Indo-European speech? Under this hypothesis, rule and order were discovered at many points where hitherto "exceptions" and irregularities had been admitted. The assumption of *e* for the parent speech comes under this head. This hypothesis (already mentioned above in connection with SCHLEICHER) was reached in the following manner. It is a fact that an *e* in other Indo-European languages often corresponds to the Indo-Iranian *a*; e. g. the *e* in φέρω and *fero* to the *a* in *bhārāmi*, the *e* in τῆ and *que* to the *a* in *ca*. Now CURTIUS showed that the European tongues usually agree in having *e* where the Indo-Iranian has *a*, and since he was as little in doubt as BOPP, GRIMM, POTT, SCHLEICHER and others that the *a* was the original vowel of the two, he assumed that this

primitive *a* had become *e* in the European parent speech. (*Ueber die Spaltung des a-Lautes* etc., in the *Berichte der Königl. sächs. Ges. der Wiss.*, 1864, pages 9—42.) This view, which long prevailed, sustained its first shock from the discovery that the Armenian shares this *e*. Ought this language to be reckoned among the European ones, or must we drop the pre-supposed originality of the *a*, and assume that an *e* or *ä* existed in the parent speech, and became *a* in those few languages which do not retain it, which *a* is not distinguished (at least in the written language) from other varieties of *a*, — so that the lack of originality would fall, not on the European, but on the Indo-Iranian side?

The truth of this assumption appears more than probable when we consider the relation of the Indian (and Iranian) palatals to the following vowels. We observe that in numberless instances a palatal springs from a guttural through the influence of a following *i* (so, for example, *ōjīyān* belongs to the positive *ugrās*). We often find that *a* has the same influence as *i*, but only the *a* which corresponds to *e* in the other languages, as for instance the *a* of the reduplication-syllable in *caḱāra*, from *kar* "make", which corresponds to *e* in Greek, German etc., or the *a* of *ca*, which answers to a Greek and Latin *e*, and so on. It follows from these facts, which could be multiplied indefinitely, that the above-mentioned *a* of the Sanskrit must have borne some resemblance to *i*, and hence that it must have been *e* or *ä*, which proves the existence of an *e* in all Indo-European languages. Now if it can be proved (and it can) that this palatalization which we find in Sanskrit must have extended back into the parent speech, the originality of this *e* (*ä*) is as surely demonstrated as it is possible to demonstrate any assertions of this nature. Without entering here into further details, I will refer the reader to an essay by JOHANNES SCHMIDT (*Kuhn's Zeitschrift*, 25, page 1 seq.), comprehending the previous labors of ASCOLI, BRUGMAN, FICK, COLLITZ and others, on the whole question of the two guttural series and the European *e*, in which he discusses the tenability of the new theory with the strictest attention to detail.

Beside the discovery of the Indo-European *e*-vowel we



may range the very probable supposition (originating with OSTHOFF) that a "sonant" (i. e. syllabic) *r*, such as we are acquainted with in Sanskrit, and which is represented in Greek by *ap* (*πα*), already existed in the primitive speech. (*Zur Frage des Ursprungs der germanischen n-Declination*, in *Paul & Braune's Beiträge*, 3, page 1 seq.) According to this view, the *a* of the Greek aorist *ἔταπον* can no longer be regarded as an isolated remnant of a former condition of the language, whose preservation is due to a certain confusion of the linguistic instinct (CURTIUS, *Grundzüge*, 5<sup>th</sup> edition, page 52), but *πα* is the regular representative of the Indo-European *r*-vowel, which is appropriate to the aorist. No less important is BRUGMAN's assumption of a syllabic nasal (*Nasalis sonans in der indogermanischen Grundsprache*, in CURTIUS' *Studien*, 9, page 287 seq.), and similar researches.

These discoveries, showing, as they did, in a most striking manner that even trifling differences in pronunciation (e. g. the difference between *ā* and *a*) are propagated with wonderful fidelity throughout the ages, contributed in great degree toward increasing the respect felt for the regularity of all phonetic change.

BOPP had granted to sounds the privilege of changing, now in this way, now in that, within the same linguistic period, and this freedom had been considerably restricted by POTT, SCHLEICHER and CURTIUS; when, now, still other important limitations were added, the idea naturally arose that *phonetic laws admit of absolutely no exceptions*. We shall show later in detail that this new theory necessarily caused greater stress to be laid upon the principle of analogy.<sup>1)</sup>

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<sup>1)</sup> The first person who, to my knowledge, clearly expressed the view that the phonetic laws admit of no exceptions, is LESKIEN (*Die Declination im Slavisch-Litauischen und Germanischen*, *Preisschriften der Jablonowski'schen Gesellschaft in Leipzig*, Leipzig 1876, pages XXVIII and 1). He says:

"In my investigations I have started with the principle that the form of a certain case, as we meet with it, can never result from an exception to phonetic laws which are observed elsewhere. To prevent misunderstanding, I will add: if by 'exception' be understood those cases where the expected phonetic change has not taken place from definite ascertainable causes, such as the absence of *Lautverschiebung* in German in phonetic

We must mention in conclusion that those scholars who advocate the infallibility of phonetic laws have often emphasized the fact that the natural constitution of language is not manifested in the cultivated tongues [*Kunstsprachen*], but in the dialects of the people. The guiding principles for linguistic research should accordingly be deduced, not from the obsolete written languages of antiquity, but chiefly from the living popular dialects of the present day.

These are the principal views, endeavors and hypotheses which usher in a new treatment of the problems of linguistic science. The chief of these problems will now be discussed in the following chapters.

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## CHAPTER V.

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### THE AGGLUTINATION THEORY.

We have shown in the previous pages how the so-called "agglutination theory" originated with FRANZ BOPP, and it has been at least intimated what part this hypothesis has

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groups like *st* etc., where one rule to a certain extent interferes with another, — then of course there is nothing to be said against the statement that phonetic laws are not infallible. For the law is not nullified in such circumstances, and works as we should expect it would do wherever these or other disturbances, i. e. the influence of other laws, are not present. But if we admit arbitrary, accidental deviations, such as are incapable of classification, we virtually confess that language, which forms the object of our research, is inaccessible to scientific investigation."

The remarks of OSTHOFF and BRUGMAN are in the same spirit (*Morphologische Untersuchungen*, 1, page XIII):

"All phonetic change, in so far as it occurs mechanically, is accomplished in accordance with exceptionless laws, i. e. the direction of the phonetic movement is always the same among all members of a linguistic community, except where a division into dialects takes place; and all words in which the sound subject to this phonetic movement appears under like circumstances, are without exception affected by the change."

Beside this we find also the extreme view that all phonetic laws work blindly, with a blind natural necessity, and the like.

played in the further development of linguistic research. It will now be my task to ascertain what degree of probability can be adjudged to it.

Every analysis of the Indo-European inflectional forms must start with the fact that certain inflectional endings of the verb show a great resemblance to certain pronominal stems. The termination of the first person, *-mi*, calls to mind at once *me*, *mi-hi* and the rest of the series, and in the same way the *-ti* of the third person reminds us of the pronominal stem *ta*, which appears in *τόν* etc. The endings of the second person also exhibit certain analogies to the corresponding pronoun, although these are not so unmistakable as in the case of the other two persons. Now BOPP explained this similarity by assuming that the pronouns were affixed to the verb, which accordingly possessed no endings before the affixion; and the idea of agglutination expressed in this hypothesis became the prevailing one in his entire explanation of inflection. But it is evident that beside BOPP's assumption it is possible to form others, with the same fact for a starting point. Thus far two such hypotheses have appeared; the first with the assumption that the endings existed first, and the pronouns were formed from these by freeing them from the stem, — the *evolution theory*; and the second, according to which the pronouns and the endings arose independently of each other, and were afterwards brought into relation, — the *adaptation theory*.

I will first discuss these two hypotheses.

The *evolution theory* is older than the agglutination theory, since FRIEDRICH V. SCHLEGEL was its first advocate; yet there exists no authentic demonstration of it, for neither AUGUST WILHELM V. SCHLEGEL, nor LASSEN, nor any other scholar of this school has opposed anything except negation to BOPP's arguments. Under these circumstances we must have recourse to the works of three men, neither of whom can be regarded as a recognized exponent of SCHLEGEL's doctrine; I refer to CARL FERDINAND BECKER, MORITZ RAPP and RUDOLPH WESTPHAL. What C. F. BECKER, the once renowned author of the *Organism*, can bring forward in support of the originality of the personal suffixes, is essentially reduced to the following observation:

"Since the word was originally a member of a sentence, the grammatical relation was given in the beginning with the notion of the word, and its inflection was given with the word itself. The word as expression of the notion, and the inflection as expression of the grammatical relation, are equally old and original."

But this reasoning would only be valid if we were obliged to assume that everything which is thought finds expression in language. Now it is evident that this is by no means the case, and therefore nothing prevents us from assuming that the idea of relation had existed long before it was expressed by linguistic means.<sup>1</sup> Accordingly, from this deductive method of observation we can draw no conclusion respecting the age of the expression of grammatical relation.

As regards the second of the above-mentioned men, MORIZ RAPP of Tübingen, I will refer the reader to a notice of his Comparative Grammar by STEINTHAL (*Kuhn's Zeitschrift*, 2, page 276 seq.), where precisely the point in question is discussed. On the other hand, the views of RUDOLPH WESTPHAL, as set forth in his *Philosophisch-historische Grammatik der deutschen Sprache* (Jena, 1865), and his *Methodische Grammatik der griechischen Sprache* (Jena, 1870), demand a more detailed consideration.

WESTPHAL's system is, in brief, as follows. In the development of language three periods can be distinguished, according to the formation of the roots. In the first, things are characterized independently, in the second, in relation to human thought, in the third, in relation to each other. (*Phil.-hist. Gr.*, page 98.) In the first period the nominal stems originated, in the second verb-inflection, in the third noun-inflection. By means of the root a name was given to *being*, as that in which a definite motion or activity is manifested. Now this root is sometimes employed to designate independent being, but usually it is phonetically changed for this purpose, being amplified by the addition of an *ä*, *ï* or *ũ*. WESTPHAL expresses himself as follows, concerning the meaning of this amplification :

"In contrast to the monosyllabic verbal root, the concrete noun thus obtains a dissyllabic form, whose terminal vowel is

only intended to indicate that the root contained in this form no longer designates *everything* in which the activity or motion in question is manifested, but a *definite* thing, or at least a definite class or department of things, of which that motion or activity is regarded as the characteristic. The enrichment of the root by an *a*, *i* or *u* only designates the progress from greater generality to more concrete individuality, to specialization."

In the course of further development the meanings of the nouns are more and more specialized, and the "nearest" [*zunächst liegenden*] vowels *a*, *i*, *u* no longer suffice; accordingly, other phonetic combinations are employed in like function. "First a nasal or dental comes before the vowel *a*, *i*, *u*", whence arise the suffixes *na*, *ni*, *nu*; *ta*, *ti*, *tu*; then the liquids, so that *ra*, *ri*, *ru*; *la*, *li*, *lu* are formed. (V. the work quoted above, page 84.) A new element is affixed to form derivative noun-stems. For every "amplification of the meaning by any characteristic or determination requires the enrichment of the existing body of the word by a new phonetic element." (Page 85.) This play of meaning and sound is most artistically developed in the verb. In the verb the following determinations find expression: 1) local identity of thinker and thought, expressed by the first person; 2) temporal identity, expressed by the present; 3) causal identity of the conceived activity and its conception, expressed by the imperative. Then we have the opposites of these determinations: 1) local non-identity, i. e. the second and third persons taken together; 2) temporal non-identity, i. e. past time (the future is not specially designated); 3) causal non-identity, i. e. the indicative. The first of these determinations is designated by the "nearest" sound, which in this case is the nasal, "whether dental or labial", the first formation being that with *m*; for example, from the root *sta* it is *stam*, while the opposite of this determination is expressed by affixion of the "more distant" *t*, i. e. *stat*. But the second person must also be specially characterized, for which purpose the "nearest" vowels *a*, *i*, *u* were at hand. It is probable that once *stata*, *stati* and *statu* could all be used for the second person; but *statu* became the favorite form. From this was formed *stas*; so we have *stam*, *stas*, *stat*. In a similar manner

the whole structure of verb-inflection is built of nothing but nearer and more distant sounds, although not without exceptions of every sort, which must appear forced and improbable even from the stand-point of the system itself. Then the pronominal stems were derived from the finished inflectional forms of the verb, especially from the forms of the middle voice. After the middle forms *tudama* and *tudatva* had arisen, *ma* and *tva* were separated from them. "In order to express the idea 'thou strikest me' or 'he struck me', the active form *tudas* or *tudat* was taken, and the 'me' belonging to the idea was designated by the same phonetic element as the reflexive 'myself' of the middle form, i. e. by the syllable *mă*." (V. the work quoted above, page 127.) The inflectional forms of the noun originated in a similar way with those of the verb, so that it is needless to discuss this part of the system more in detail.

This system challenges criticism in many respects, in the first place as to its philosophical basis. I think all must allow that the latter has nothing in its favor but a certain grandeur of terminology. It seems to me that a sensible philological public will be no more ready to admit that these abstruse and obscure "determinations" entered into the heads of our forefathers as language-forming forces (and that, too, in a precise categorical succession), than it will to believe the author when he declares that the same primitive forces "lie at the foundation of sidereal, vegetable and animal existence." A further objection lies in the doctrine of the nearer and more distant sounds. Leaving out of account the fact that WESTPHAL continually contradicts himself in his conception of the distance of sounds, what does it mean to say that one sound is near and another far off? Among the consonants he supposes the nasals and dentals to be the nearest; are we to assume that these arose first, and that the labials, for example, are of later date? Other phonetic assumptions are extremely doubtful. How is it to be explained, for instance, that the consonants *t*, *n*, *r*, *l* make their appearance in the middle of a word before the suffix-vowels *a*, *i*, *u*? Where can we find anything analogous in the domain of Indo-European language?

But the point which especially interests us here is the

theory that the personal suffixes were separated from the stem. Is this theory probable? The objection may be made, we must then necessarily assume that the Indo-European languages had managed for some time to do without personal pronouns. And this assumption is, in CURTIUS' opinion (*Verbum*, I, 2<sup>nd</sup> edition, page 22), a very forced one. For where, he asks, are there languages without personal pronouns? Then, too, we must admit that the whole idea of the endings "falling from the tree like ripe pears" (PORT, *Etym. Forsch.*, 2, page 360), or "exuding like rosin and falling to the ground" (as SCHERER expresses it, is peculiar and without analogy. At least to my knowledge nothing corresponding has been adduced from other languages, while at all events the example of agglutinating languages (as we shall see later) can be quoted in favor of BOPP's hypothesis.

I can therefore hardly think that the evolution theory, in the form in which it has hitherto appeared, has any claim to the approbation of philologists, — the less so, if in the course of this demonstration it appears that there is considerable probability on the side of the agglutination hypothesis as a whole, although not in its details.

We come to the *adaptation theory*, or the views which ALFRED LUDWIG brought forward in his essay on the origin of the *a*-declension, in the *Sitzungsberichte der Kais. Akad.* (Vienna, 1867), and in the two separately published articles: *Der Infinitiv im Veda nebst einer Systematik des litauischen und slawischen Verbs* (Prague, 1871), and *Agglutination oder Adaptation? eine sprachwissenschaftliche Streitfrage* (Prague, 1873).

A. LUDWIG, an admirable Vedic scholar, is of opinion that the prevailing ideas regarding the constitution of Indo-European language have been too exclusively based on the Greek. The Veda ought to be used far more extensively, for from the Vedic language alone can we derive sure guides to a correct conception of the inflectional endings, both of the verb and the noun. As to the verb, it is a fact that in the Veda the third person singular of the middle sometimes has the same ending in the present as in the perfect, i. e. *-e* (not *-te*), and is also identical with the first person singular, so that *çr̥ṇré* can mean "he is heard", as well as "I am heard". LUDWIG

finds something corresponding in the second person middle, assuming that the suffix *-se* was used with the signification of the first as well as second person. Now by drawing conclusions from *-e* and *-se* with regard to *-te*, and then further with regard to *-mi*, *si*, *ti* (where he thinks that the same variety of meaning is not so unmistakably evident as with *-e* and *-se*), he arrives at the opinion that originally the so-called personal suffixes had nothing to do with the designation of person. There were, accordingly, no original personal suffixes, but only one single class of suffixes, i. e. those which we call "stem-forming". The forms of the finite verb are in their origin nothing but stems. The same is true of noun-inflection. LUDWIG tries to prove with the help of the Veda that the cases originally had no separate sphere of meaning. It is nonsense to talk of the primitive meaning of a case. In that field, also, which we call "nominal", there were originally only stems, whose meanings were gradually differentiated and specialized.

But on the other hand, LUDWIG keeps in view the fact that in the later periods of linguistic development, for example, in classical Sanskrit, each of the various endings really indicates a particular mode of employing the word. So the question arises: how have the suffixes obtained this meaning, which they once did not possess? The answer is: they were invested with it. The growing intellectual need demanded expression for certain categories, and the suffixes, which originally had solely a demonstrative sense, adapted themselves to this need. Last of all arose the forms of the finite verb, whose immediately preceding stage is formed by those stems which we now call infinitives. That the changes above intimated may be better understood, I will let the author himself speak. After demonstrating that the dative and locative, considered from a historical stand-point, lose their quality of inflected forms and "step back into the realm of word-formation", he continues:

"This process of word-formation was gradually arrested, and beside it appeared another tendency, namely, to turn to account those forms which were divested of their original meaning. In the beginning men neglected to especially characterize *agens*, *actio*, *actum*, and contented themselves with



the employment of demonstration, which was evidently used at that time to a very large extent. As soon, however, as language possessed suitable material, it went to work (although by no means consistently) to introduce this distinction, which promotes perspicuity of speech to such an extraordinary degree. When this differentiation had reached a certain point, there was undoubtedly another inclination to indicate the relations of number and case; but even for this purpose only existing materials were used, and we must not suppose that a grammar was created". (*Inf.*, § 19.)

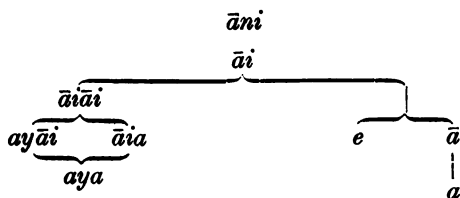
In another passage we read: "What was required to produce the impression (however indistinct) of inflection? Nothing except forgetfulness. In the formations in question, as long as the actual connection of things remained in the memory, there were only stems, no inflected stems. As soon as the remembrance of this connection had vanished, the necessity arose of framing some opinion concerning, or of really understanding, these differences (with regard to whose true nature and origin nothing was any longer known, nay, concerning which it was not even known that there was anything to know); for it was doubtless imagined that the forms were understood when they were invested with a meaning". (*Inf.*, § 29.)

And some pages farther on: "With the gradual growth of the forms two phenomena very naturally made their appearance, which became the poles on which syntax revolved (indeed, we can say that syntax did not previously exist at all, except in phraseology), — viz., the designation of grammatical correspondence, or grammatical subordination and coördination. When different expressions stood in any sort of connection, it was natural to attempt to denote this in a way that should characterize the difference or identity of the relation of several expressions with reference to another. The further consequence of this was that a certain need for so-called grammatical endings was felt, and the simple stem-ending either gradually fell into disuse, or, being confined to a special field of meaning, assumed the appearance of an inflected form. Certain endings appear to have been too much in favor: *ām* loc. sing., gen. pl., nom. acc. du., and, as we are convinced,

also instrum. sing. (*ā*), cf. Old Slavonic *ajq*; and the same with *bhi*. In this way words for the first time seemed rounded off and completed. At the same time, in proportion as the demand increased, the number of possible word-endings became limited." (*Inf.*, § 31.)

Add to this a passage from LUDWIG's controversial essay: "As their original meaning [i. e., of the personal suffixes] I assume the demonstrative meaning, which first gave place to the function of word-formation; then they acquired a general verbal meaning [such as appears in the infinitive], and as the number of these elements increased, they were finally, according to incidental analogies (or often no analogy at all), brought into connection and relation with the categories of grammatical person, which had been meanwhile developed in the personal pronoun. I accordingly assume an original meaning, and in addition the passage through three metamorphoses." (*Agglutination oder Adaptation*, page 62.)

If this demonstration has enabled the reader to form an approximate idea of LUDWIG's general views, there remains the important task of showing how LUDWIG acquired these views from the actual constitution of the Indo-European sounds and forms. Of course it is not possible for this purpose to follow the author into every detail; hence I will simply remark, in general, that LUDWIG imagines he has discovered a number of phonetic laws which differ considerably from what is looked upon by other scholars as established. For example, he thinks himself justified in assuming that in the Indo-European every suffix ended in a vowel; that *t* was changed to *s*, and *s* to *r*, *t* passed into *n*, *n* was dropped out between vowels, etc. To illustrate this procedure, I will mention as an example that a stem in *-āni*, which is used after the manner of an infinitive, is supposed to have undergone the following changes:



Here the *e* is what we call the first or third person in *e* (e. g. *gr̥vé*, v. above, page 66); by *ā* are meant the forms in *ā* like *stāvā* etc., which are known to Vedic scholars; by *a* the stem of the verbs of the *a*-conjugation. Such forms were in LUDWIG'S opinion used a long time in a verbal sense, without further endings (such as we call personal endings); afterwards forms like *bharā* and *bhara* received the suffixes *mi*, *si*, *ti* etc., through transfer from verbs like *doṣi*, where the stem-endings *mi* etc. had adapted themselves so as to become a sort of personal suffix.

In order to estimate the plausibility of these hypotheses, we must first of all form an opinion regarding LUDWIG'S view of the language of the Veda, for it is clear that the adaptation theory would receive a powerful support if the diversity of meaning of the Vedic forms, asserted by LUDWIG, could be established. I formerly expressed the opinion that this proof has not been and cannot be given (*Kuhn's Zeitschrift*, 20, page 212 seq.), and I hold to this view the more, because it is precisely within the last few years that the progress in Vedic interpretation (in which LUDWIG himself has had no insignificant share) has shown more and more plainly that LUDWIG'S assumptions can be dispensed with. Now if this support is withdrawn from the adaptation theory, its inner probability is the only proof which remains, since LUDWIG'S phonetic laws themselves have no other foundation than the probability of the theory. How is it with this inner probability? I can hardly regard it as a warrantable proceeding to reject from the whole field of grammar the idea that inflectional suffixes were derived from stem-suffixes (we shall meet with this theory again in connection with the noun), but LUDWIG'S application of it to the verb seems to me unjustified. Even if we were willing to admit that the persons of the verb have arisen from stems by means of differentiation, which seems to me very improbable, the question would always remain unanswered, whence comes the resemblance of the so-called personal suffixes to the pronouns, a resemblance which cannot be denied. LUDWIG'S answer to this question looks very much like a confession of ignorance. I would call the attention of the reader to one of the sentences quoted above, which runs as follows:

"As the number of these elements increased, they were finally, according to incidental analogies (or often no analogy at all), brought into connection and relation with the categories of grammatical person, which had been meanwhile developed in the personal pronoun." If I am not mistaken, when the author in this sentence allows that the relation between suffix and pronoun can arise "according to no analogy at all", he renounces every attempt at explanation in one of the most important points of his system, and thus himself formulates the weightiest objection to his hypothesis. The adaptation theory, which assumes the independent origin of the personal suffixes and the pronouns, must above all be able to demonstrate, or at least to indicate with some degree of probability, how, in spite of their independent origin, the striking similarity of the elements in question can be explained. And this demonstration LUDWIG has not given. Hence I am able to ascribe as little probability to the adaptation theory, taken as a whole, as to the evolution theory.

Let us now see what follows from the rejection of the two hypotheses above mentioned. We have observed that every theory of the origin of inflection must be based upon the similarity between certain personal suffixes and certain pronouns, which is so great as to exclude the possibility of an accidental cause. So far as I see, this similarity can be explained in three ways. Either we assume that the endings arose from the pronouns, or that the pronouns arose from the endings, or, finally, that endings and pronouns arose independently and were afterwards made to resemble each other. The second and third assumptions seem to me improbable, as I have just stated. If, then, we are not disposed to renounce every attempt at explanation (a stand-point which will receive due attention at the close of this chapter), we are reduced to the first hypothesis, — that of BOPP. This also receives support from another direction, i. e. from the analogy of the so-called "agglutinating" languages.

In this field I cannot judge from personal observation, and must therefore depend wholly upon the representations of one who is versed in these languages, namely, BÖHTLINGK, in the introduction to his Yakut Grammar. I am unwilling to

mutilate his concise development of the subject by an extract, but will recommend to the reader the study of this instructive treatise, which is no longer used as much as it deserves. But in order to give an idea of what I mean by my reference to BÖHTLINGK, I will quote one passage word for word (page XXIV):

"If we compare all the phenomena, we must admit that in the Indo-European languages in general, material and form are far more intimately united than in the so-called agglutinating languages, although in certain members of the Ural-Altaic family, especially in the Finnish and Yakut, the connection of material and form is not so wholly superficial as POTT and other philologists are inclined to assume. I must also freely confess that on the whole I consider the way in which material and form are combined in different languages as a too external characteristic to serve as the basis for a classification of language. The looser or closer combination of the material with the form stands in intimate connection, not only with a nation's capacity of articulation, but also with the age of the forms and the frequency of their use. I am strongly convinced that in the Indo-European languages, which in regard to this combination stand on a higher plane than, for example, the Ural-Altaic, the form-making process began much earlier than in the latter languages. Within the Ural-Altaic family I believe that the Finnish reached the form-making stage earlier than the Turko-Tartaric, and this latter earlier than the Mongolian. In the oldest linguistic monuments of the Indo-European nations we find the grammatical forms on a plane of development beyond which no further progress has been made; what has newly arisen on the ruins of these forms must be regarded as a new creation of forms within the history of these languages. The Ural-Altaic languages, perhaps with the exception of the Finnish, have not yet attained the culmination of the first form-making process; if among them we meet with uninflected words, these are remains of an older period of the language, where inflection was not yet developed; on the other hand, the uninflected words of the newer Indo-European tongues are, as a rule, decayed inflectional forms. A comparison of the Mongolian and Kalmuck popular

dialects with the written language shows us quite plainly how forms originated in the most recent past. The Mongolian written language knows no affixed pronouns, either possessive or predicative; in the present language of the Buriats both sorts of affixed pronouns (although not in wholly distinct forms) have developed, so that in the verb a variation takes place according to person. We observe the same phenomenon with the Kalmucks: *üsüdshi bainu tschi* 'seest thou' is in the vernacular contracted into *üsüdshünütsch*; *ögüngüdshi bainai bi* 'I shall soon go', 'I am on the point of going', into *ögüngüdshünüb*. In a similar way the preposition *ätsü* is combined with its noun so as to form an inseparable unity, and becomes an actual case-ending: *chagāsa* 'whence', in the written language *chamigha ätsä*. We see from this how over-hastily the conclusion was drawn from the fate of the Indo-European languages that the history of language, so far as it is the history of the development of language-formation, preceded universal history."

The conclusion of these remarks is of especial interest for the question at issue. For the observation that linguistic forms arise within historical times by means of composition would necessarily have great weight toward establishing a similar assumption for the so-called "pre-historic" period.

It is true that everything which has here been adduced in favor of BOPP's view can only serve to recommend the principle in general. How far this principle will be found true in detail can only be decided by a special discussion, to which I now pass. I will make three chief divisions: roots, the noun and the verb.

## I. ROOTS.

### A. *The idea of the root.*

As has been shown above, BOPP derived from the grammatical tradition of his time, as well as from the Indian grammarians, the principle that the whole word-material of a language must be traced back to roots. But whether these so-called roots shall be regarded as real linguistic structures, or only as abstractions of the grammarian, BOPP, who is never

over-fond of general discussions, has not, so far as I can find, expressed any opinion. On the other hand, the question has been thoroughly discussed by PORR, in the first edition of his *Etymologische Forschungen*, in various passages, and in the second edition, in a thick volume of over a thousand pages (2<sup>nd</sup> edition, 2<sup>nd</sup> part, 1<sup>st</sup> section: Lemgo and Detmold, 1861). His opinion, expressed as nearly as possible in his own words, is as follows:

"Roots are the chieftains of a word-family; they are the unity, the pyramidal point in which all members of such a family terminate; only composita can, like married pairs, belong to two families. Roots are furthermore only imagined, a mere abstraction; in reality there can be no roots in language, — whatever may wear the outward appearance of a pure root is a word or a word-form, not a root; for a root is an abstraction of all word-classes and their differences, a focussing of them without refraction" (1<sup>st</sup> edition, page 148). Similarly, in the second edition: "A root is not, like a letter or syllable, simply the phonetic unity, it is also the unity of meaning of words and forms which genetically belong together, and at their creation was present as prototype in the soul of the language-maker; where not wholly obscured, it is felt more or less plainly by every speaker, in connection with the language (usually the mother-tongue) which he uses." Add to this, page 194: "Roots are ever mere ideal abstractions, necessary to the grammarian in his calling, which he must nevertheless extract from language in strict conformity with the given reality." PORR accordingly denies that roots can have existed before inflectional forms: "If, now, it must be asserted that declension arises in the Sanskrit languages by the affixion of inflectional suffixes to the fundamental forms of the noun, and conjugation through affixion of others to the root or stem, this must not be misunderstood, so as to imply that the fundamental form and the root are something existing independently and out of connection in language, or something as it were present in language before inflection; what is really meant is only that the fundamental form is contained in all the cases of nouns, and the root in all verbal forms, as that which is still undifferentiated, as that which is common

to them, which grammatical analysis alone, for scientific ends, tries to free from all the differentiating characteristics united with them, and to display in its simplicity" (1<sup>st</sup> edition, 1, page 155); and similarly in the second edition, page 196 (cf. also 1<sup>st</sup> edition, 1, page 179).

This definition of POTT is correct in so far as it rightly defines the position a root occupies within a finished inflectional language, but it is one-sided inasmuch as it does not state how the root arrived at this function. To this question only one answer is possible from the stand-point of BORP's hypothesis. If the prototypes of the now existing inflectional forms really arose by means of composition, especially the prototypes of the forms of the finite verb by composition of a verbal with a pronominal root, then the root must have existed before the word originated. Roots are contained in words because they existed before them and were merged in them. They are the words of the pre-inflectional period, and vanish with the development of inflection. Therefore, from the stand-point of the perfected inflectional speech, what was once an actual word appears only as an ideal centre of meaning. This wholly intelligible and consistent notion of the root may be said to be universally accepted at the present day. Cf. on this point what CURTIUS brings forward in his *Chronologie*, 2, page 23, and especially BENFEY, *Gött. Gel. Anz.*, 1852, page 1782, as well as STEINTHAL, *Zeitschrift für Völkerpsychologie*, 2, pages 453—486.

It seems as if even POTT in the end might favor this view. And we do find in his writings passages like the following:

"It is conceivable that the Sanskritic languages, as they are transmitted to us, were preceded by a condition of the greatest simplicity and absence of inflection, such as is exhibited even today by the Chinese and the other so-called 'monosyllabic' languages." (*Etym. Forsch.*, 1<sup>st</sup> edition, 2, page 360.)

If POTT notwithstanding holds himself aloof from the historical conception of the root as above described, this is evidently due to a critical disinclination to all reconstruction of the parent speech. But this disinclination goes too far when it opposes not only the establishment of roots in single cases, but also the whole notion of the root as the word of the



primitive period. For this idea of the root is a necessary consequence of BORR's theory of composition, to which POTT also adheres.

From the above idea of the root a consequence at once follows which is of practical importance. If the roots were no longer in existence in the individual languages, no longer even in the inflected Indo-European language, but only in the period which lies behind it, then we cannot speak of Sanskrit, Greek, Latin, German, Slavonic etc. roots, but only of Indo-European ones. If, notwithstanding, we postulate roots for the individual languages, they have no scientific value, but only the significance of practical aids. In this respect the antiquity of the separate languages makes no difference. Sanskrit roots can no more be justified than New High German or Roumanian ones, for the circumstance that in the ancient languages the original roots can be more easily recognized has nothing to do with our theoretical judgment. The historical relation is everywhere the same; at an infinite distance back of all tradition lies the time in which Indo-European inflection did not exist, in which *dā*, we can say, was used to express "give", "giver" etc. Then when a *dāmi* "I give", a *dātar* "giver", etc. were formed, the root *dā*, as such, had vanished from the language. From that time forth (after the completion of inflection) no longer roots, but only words existed. And when finally (probably thousands of years after) individual races, as the Hindus, Greeks etc., were separated from the primitive race, they of course carried nothing away with them from their original home except actual words. In many words what had been the root was still plainly preserved, for example in the Greek δίδωμι, δοτήρ etc., and these words naturally formed an associated mass in the mind of the speaker; but a root *do* or *δω* did not exist in the language of the Greeks. In other cases, on the contrary, even in languages of as great antiquity as the Greek, kindred words are no longer held together by phonetic similarity. The Hindu may have still been conscious of a connection between *ācūs* (ᾠύς) and *ācvas* (ἄκρος), but the Greek certainly could no longer feel the slightest connection between ᾠύς and ἄκρος. Now the modern languages only differ from the Sanskrit, Greek etc. in

this, that the relation which we find exists in Greek in the case of ὄνυς and ἵππος has with them become much more frequent.

Although it is thus clear that it is unscientific to speak of roots in the individual languages, it is nevertheless probable that owing to their convenience they will not disappear from practical use in linguistic science. And there is really no objection to the employment of illustrative aids, so long as they are not confused with realities. In postulating these roots the form is naturally of little moment. Whether we say φερ, or φορ, or φαρ, or finally φρ, is simply a matter of agreement.

### B. *The classes of roots.*

BOPP expresses himself as follows in regard to the classes of roots :

"In Sanskrit and its kindred languages there are two classes of roots ; from one of them, by far the larger of the two, arise verbs and nouns (substantives and adjectives), which latter stand in a fraternal, not in a derivative relation to the verbs, since they do not originate from them, but are born of the same parent as they. We call all of this class, however, 'verbal roots', for the sake of distinction, and according to prevalent custom. From the second class arise pronouns, with all conjunctions, particles, and original prepositions ; we call these 'pronominal roots', because they all express a pronominal notion, which in the prepositions, conjunctions and particles is more or less concealed."

A number of scholars have adopted this classification (cf. G. CURTIUS, *Zur Chronologie der indogermanischen Sprachforschung* [*Abh. der phil.-hist. Classe der sächsischen Ges. der Wiss.*], 2<sup>nd</sup> edition, Leipzig, 1873, page 23 ; and WHITNEY, *Language and the Study of Language*, pages 258, 261), although some of them have preferred other appellations for the two classes, among which MAX MÜLLER's terms "predicative" and "demonstrative" seem to me the most satisfactory.<sup>1)</sup>

<sup>1)</sup> CURTIUS' term "naming" [*nennende*] roots (for predicative) is shown to be impracticable when we attempt to develop the parts of speech from the two classes of roots. For if we derive verb and noun from the

On the other hand, the following objections have been raised against BOPP's view:

In the first place, it has been doubted whether an original duality of classes can really be assumed, and whether the demonstrative class should not rather be derived from the predicative. Of this opinion are such scholars as JACOB GRIMM, SCHLEICHER (cf. CURTIUS' *Chronologie*, 2<sup>nd</sup> edition, page 24) and WEBER (*Indische Studien*, 2, page 406). They derive, for example, the pronominal stem *ta* from *tan* "stretch", and *ma*, the pronoun of the first person, from *mā* "measure" (where SCHLEICHER assumes the following development of meaning: "measure", "think", "man", "I").

SCHERER follows them in part; he says (*Zur Geschichte der deutschen Sprache*, 2<sup>nd</sup> edition, page 451) that something of what WEBER asserts on this point will probably hold good; but he differs from the above-named scholars in also assuming a derivation of predicative roots from conceptions of space.

I can see no probability in any of the derivations brought forward, and am inclined to hold fast to the conviction that a unity above and back of the duality asserted by BOPP has not yet been plausibly demonstrated.

BENFEY has expressed a peculiar view, which partially coincides with that just mentioned. He, too, assumes that the predicative roots were the foundation of all roots, but defines them more narrowly than BOPP and the other philologists. For while BOPP conceives that noun and verb are born as twins from the predicative roots, BENFEY regards the verbs alone as primitive, giving no longer the name of roots, but of primitive verbs, to the original monsyllabic elements, which he, too, assumes. He therefore derives the whole mass of Indo-European words from primary verbs. This theory is chiefly supported by BENFEY's theory of suffixes. And since, as I shall show farther on, I cannot approve the latter, I am unable to accept its consequence, the monsyllabic primary verbs.

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"naming" roots, and then give as characteristic of the noun that it "names", while the verbs "affirms", we use the term "name" in two different technical senses.

The views hitherto mentioned have this in common, that they are more or less definitely inclined to replace BOPP's assumed duality by a unity. But an opposite objection can also be raised. Do BOPP's classes suffice? Can all the traditional parts of speech, without exception, be derived from them? Such an attempt at derivation is attended with serious difficulties in the case of the prepositions and particles, leaving out of account the numbers, whose origin is unknown. POTT places the prepositions in neither of the two classes, but thinks they are *sui generis*, and as original as the pronouns. I do not believe it will be possible to analyze with any degree of certainty the original prepositions of the Indo-European (I cannot approve the experiment of GRASSMANN in *Kuhn's Zeitschrift*, 23, page 559 seq.), but it is nevertheless clear that in point of meaning they stand in near relation to the pronouns, and therefore it may be legitimate to bring them into a class with the latter. There are greater difficulties, however, in the case of certain particles, for example the particles of repulse and encouragement, *má* and *nú*. It is not easy to see how these words, which neither characterize a phenomenon, nor bring the speaker into momentary connection with his surroundings, can be brought under one of the existing heads. Perhaps a third class of roots should be assumed, i. e. those roots which appear as the accompaniment of more general sensations, and belong with the interjections, which cannot be wholly excluded from language.

It is difficult to attain to sure results in this field by means of inductive linguistic research, although we shall probably advance somewhat beyond our present stand-point, as soon as the subject of the parts of speech shall receive more serious attention. Moreover, room must always be allowed for the consideration of psychological probability, and thus the whole question will claim a different and more comprehensive discussion than I can undertake here.

### C. *The form of roots.*

In regard to the form of roots, BOPP says that except the law requiring them to be monosyllabic, they are subject to no

further limitation. BENFEY, CURTIUS and others are of the same opinion, and SCHLEICHER adds the condition that a root may never contain a strengthened sound, but only one of the fundamental vowels (*a*, *i* or *u*).

A reason which we may almost term philosophical is first of all adduced to explain the fact that roots are exclusively monosyllabic; this is expressed as follows by ADELUNG:

"Every root-word was originally monosyllabic, because man while still in a rude state of nature brought forth his whole conception with one opening of the mouth."

WILHELM VON HUMBOLDT expresses himself in a somewhat finer vein:

"But if we regard the question simply from the standpoint of ideas, it is not going too far to assume that originally every notion was designated by one syllable alone. 'Notion' in the process of inventing speech is the impression which the object, either external or internal, makes upon the man; and the sound called forth in consequence of the vividness of this impression is the word. Thus two sounds cannot easily correspond to one impression." (Quoted by POTT, *Wurzeln*, page 216.)

CURTIUS says in the same spirit (*Chronologie*, page 23): "I am also in harmony with most philologists in my assumption that roots were monosyllabic. Swift as lightning, it has been said, the unitary conception bursts forth in a complex of sound which must be audible at one instant."

It is evident that such a reasoning, however plausible, can have no binding force, and the whole question is reduced to this, whether there is an empirical proof for the assumed monosyllabic form. The root is found by stripping off all the formative syllables of a word. If, now, it follows throughout that the kernel which remains after this operation is monosyllabic, our hypothesis is proved. But this proof moves in a circle. Root is what is not formative syllable, and formative syllable is what is not root; but where the line is to be drawn between the two must be decided by our grammatical reasoning. What, now, if this reasoning were at fault, and if we, for example, ought to divide *gámati* "he goes" not *gam-a-tí*, but *gama-tí*, that is, assume a dissyllabic root?

How far the above hypothetical observation might possibly be correct, we can perhaps learn from the investigations which have recently been instituted concerning the history and development of roots.

There can be no doubt that the roots which we are accustomed (mainly according to the example of the Indian grammarians) to postulate as Indo-European do not all stand upon the same historical plane, but that among them we must distinguish between older and younger formations. In attempting to do this, PORT adopted a method which has now (in my opinion rightly) been abandoned; he assumed that prepositions or other prefixes are often contained in the first sounds of the roots; so, for example, he explains *svād* "enjoy" as made up of *su* "good", *ā* "to" and *ad* "eat". (Cf. the polemic against this mode of explanation in CURTIUS' *Grundzüge*, 5<sup>th</sup> edition, page 32 seq.) CURTIUS adopts the opposite method, by frequently separating the final consonants as later additions, so-called "root-determinatives"; so, for example, he derives *yudh* "fight" and *yug* "unite" from a common primitive root *yu*, without, however, giving any definite opinion as to the nature and origin of these determinatives. FICK followed in CURTIUS' foot-steps, and undertook a very comprehensive analysis of roots in the section of his root-lexicon which bears the title: "Roots and root-determinatives". There he arrives at the following general result:

"The primitive root can consist of: 1) a single vowel (*a, i, u*); 2) *a*-vowel + consonant (*ad, ap, as*); 3) consonant or double-consonant + *a*-vowel (*da, pa, sa; sta, spa, sna*). All roots which have other or fuller forms either arose from the primitive roots by phonetic weakening (e. g. *ki* from *ka*, *gi* from *ga*, *tu* from *ta*), or are further formations from them by means of the affixion of determinatives."

He tries to adduce an empirical proof for this assertion, by showing that all, or almost all roots whose form does not come under the three categories mentioned above, can without difficulty be traced back in form and meaning to roots which are in conformity with these three norms.

To show how he conducts this process I will give an example:

*ka* "sound".

*ka, ka-n canere*, "sound", "resound".

*ka-k* "laugh".

*ka-t* "be noisy", "chatter".

*ka-r* "call", "name".

*kar-k, kra-k* "resound", "laugh", "crash", =  
*kru-k* id.

*kar-d, kra-d* "rustle", "resound".

*kra-p* "be noisy", "wail", "be wretched", cf.

Skr. *karuṇa* "wretched".

*kru* "hear", cf. Aryan *krat-u* "insight".

(*kru-k* "cry", "crow", "croak", probably derived from *krak*.)

*kru-s* "hear".

*ka-s* "point out", "extol", "praise".

*kās* "cough".

*ku* "cry", "howl".

*ku-k* "cry", "howl".

*ku-g* "whine", "chirp".

*ku-d* "be noisy", "revile".

FICK has recently (*Bezzenberger's Beiträge*, 1, page 1 seq.) modified this theory to a considerable degree, and now discovers remnants of syllables in all the "determinatives" assumed by him:

"If forms like *mak*, *star*, *dam* were formed by the composition of the primary roots *ma*, *sta*, *da* with a second member, it is quite impossible to doubt that the products of this composition must have been originally *ma-ka*, *sta-ra*, *da-ma*; for since elements like *k*, *r*, *m*, i. e. simple consonants, do not exist in the Indo-European, they can never have been employed for purposes of derivation".

Therefore it is FICK's opinion that (to repeat the example given above) *gámati* must be resolved into *gama-ti*, and that *gama* must be regarded as a dissyllabic secondary root, which was formed from the original root by the affixion of *ma*.

Before FICK, ASCOLI<sup>1</sup>) (in his *Study ariosemitici*, 1865)

<sup>1</sup>) It is only on account of FICK's detailed demonstration that I have judged it expedient to bring his arguments first before the attention of the reader.

had brought forward essentially the same arguments, which he has recently taken up again in his introductory letter on the palaeontological reconstruction of language, in his *Kritische Studien* (Weimar, 1878). There he says (page XXXI):

"We find at the same time that very many radical combinations in the Indo-European lexicon, instead of remaining true to their old significance as genuine first elements, genuine roots or original monosyllables, allow of an accurate analysis, by which they are found to be compositions of a really original monosyllable with one or more affixed elements, of derivative, determinative or supplemental nature, as we are pleased to call them. Thus these apparent roots are reductions of dissyllabic (or even trisyllabic) aggregates, reductions or inner kernels, which never possessed an actual, independent life, but were only obtained through the union of the old aggregates with new accessory elements of different derivative or inflectional meaning. So it is true that in the language of the Indo-Europeans before their separation the phonetic group SKID ('cut', 'split', Latin *scid-*, Zend *scid-* etc.), with the vowel *i*, did really exist, but we find at the same time unmistakable successors of the synonymous SKAD (Zend *skeñda* etc.), and of the also synonymous SKA (SAK-A; Sanskrit *chā*, Latin *sec-*); and we must in fact go back from *skid* to a *ska-da*. For 'run' the Indo-Europeans before their separation had a phonetic group DRAM (Skr. *dram*, Gr. *δρεμ-*), which, however, is really DRAMA; DRA occurs in the synonymous *drā* of the Sanskrit and Greek (*ἔ-δρα-ν*); a third synonym, the Sanskrit *dru* (*drava-ti*) certainly cannot claim that its *u* is original. The accessory element of DRAM appears in TRAM (TRA-MA; Lat. *trem-* etc.), whose true radical foundation occurs again in the synonymous group TRAS (TRA-SA; Skr. *tras*, Gr. *τρεσ-*, *τρέω*), and also in TRAP (TRA-PA; e. g. in Lat. *trepidus*). Similarly, for Skr. *kṛt* 'cut' (cf. *κρίνω*) we must go back to KARTA (= KARA), or for Zend *staχ-ra* 'what offers resistance, stands firm' to STA-KA, and so on in countless other cases."

In our judgment of these views we must bear in mind the following considerations. From the existence of both *yug* and *yu*, *dram* and *drā* etc., we are easily led to suppose that roots have



been amplified by the affixion of new elements. That these elements originally consisted not of single sounds, but of syllables, is also a very natural assumption. There are therefore no valid objections to bring against the supposition that dissyllabic roots may have existed by the side of monosyllabic ones.

We do, indeed, meet with great difficulties in single instances. For example, there may be a difference of opinion as to the method of explaining the second *a* of the Sanskrit present *gámati*. Are we to assume that *gama* in *gámati* is the ancient dissyllabic root-form, or had *gama* already become *gam* in the pre-inflectional period, and was the present *gam-a-ti* then derived from it, with a suffix *a*, whose existence FICK does deny, but which, as I shall show below, it is impossible to avoid assuming? It seems to me that the greatest difficulty lies in the meaning. Can we assume that the oldest roots had a meaning so general and so indefinite as "sound"? Is there not, on the contrary, every probability that such conspicuous phenomena as rustling, singing, laughing etc. (the designations for which FICK derives from the designation for "sound") were the first to find expression in language?

But it is not my aim here to speak more in detail of these investigations, which are only in their infancy. I merely wished to show by what arguments a modern philologist can arrive at the postulation of dissyllabic roots by the side of monosyllabic ones.

I would like, in conclusion, to say a word of SCHLEICHER's opinion concerning the vowel in the root. We have derived from the Indian grammarians the view that diphthongs can be formed from the primitive vowels *i* and *u* by means of strengthening, and have also followed their example, for the most part, in ascribing the simple vowels to the roots; e. g. *i* "go", not *ai* (or *ei*); *ruk* "shine", not *rauk* (or *reuk*). But here comes a difficulty. If from the present forms *eimi*, *imás* we extract the root *i*, we ought consistently to assume a root *s* for *ásmi*, *smás*. (Cf. BEGEMANN, *Das schwache Präteritum*, Berlin, 1873, page IX seq.) Now root-forms such as *s*, *pt*, *bhs* had certainly made their appearance in the words of the primitive speech (cf. BRUGMAN, *Morphologische Untersuchungen*, 1, page 11); but it is impossible to believe that they could

have been present in the root-period as independent linguistic elements. We are rather forced to assume for the root-period the forms *as*, *pat*, *bhas*, or *es* etc., and therefore not *i*, but *ai* (or *æ*). This involves an inversion of the previous theory of vowel-strengthening, which in fact has taken place in the case of several scholars. (Cf. PAUL & BRAUNE, *Beiträge*, 6, page 408.) Yet a systematic demonstration is still wanting. For our present purpose, we can at least draw the conclusion that SCHLEICHER's view of the root-vowel cannot stand as an inevitable and final result.

So much for the form of roots. The most important points for the reader to bear in mind are the following. We have only words. We extract the roots from them by grammatical operations. But in these we can err, and opinions may change as to what is correct and what false. The same is accordingly true of the form of roots as of the form of the words in SCHLEICHER's parent speech. If BOPP's analysis in general holds good, it is certain that so-called "roots" were the words of the primitive speech in a period previous to inflection; but the form attributed to the individual roots merely exhibits the opinion of scholars regarding the method of analyzing the transmitted words of the Indo-European languages.

## II. THE NOUN.

### A. Stem-forming suffixes.

It is well known that in the Indo-European there are noun-forms originating from the immediate affixion of the case-sign to the root, e. g. *duc-s*; while the majority have certain elements between the root and the case-sign, which we call stem-forming suffixes. These consist now of a simple vowel, now of a consonant and vowel, like *ta*, *ma*, *ra*, or of a vowel and consonant, like *as*, or they have a fuller form, like *tar*, *tama*, *mant* etc. BOPP's opinion concerning those suffixes which consist simply of *a*, *i* or *u* was at first undecided, and somewhat in accord with SCHLEGEL's view, as we see from a passage in an academical essay of July 28, 1831 (page 15):

"The meagre form of these suffixes leads us to easily overlook the ancient composition, in the case of those verbal roots

which by their agency are made into words, introduced to life and clothed with personality. It may be preferable to regard these sounds as the feet, with which a root is endowed, or which have grown to it in order that it may move upon them in its declension; they may also be regarded as spiritual emanations of the root, which have come forth, no matter how, from its inner being, and have but the semblance of individuality, since they are really one with the root, or merely its organically developed flower or fruit. But I prefer the explanation which is the simplest, and which is supported by the genesis of other linguistic families <sup>1)</sup>; and since nothing is more natural than that word-formation, like grammar in general, should, on the whole, depend upon the union of one significant element with another, it seems to me hardly possible to doubt that the *a*, for instance, in  $\text{dam-a}$  'subduing', 'subduer' is intended to represent the person who possesses or exercises that quality which is designated by the root  $\text{dam}$ ;  $\text{dam-a}$  is therefore as it were a third person of the verb, in a nominal (i. e. substantive or adjective) state, independent of time-determinations."

This theory is brought forward with greater certainty (as remarked above) in the Comparative Grammar, where the majority of the stem-forming suffixes are derived from pronouns, while he attempts to trace a portion (e. g. *-tar*) back to predicative roots. POTT follows BOPP's opinion in the main (*Etym. Forschungen*, 1<sup>st</sup> edition, 2, pages 454 seq.). SCHLEICHER and CURTIUS differ from him in giving up the derivation from predicative roots; for example, they would assert that *tar* was made up of the two pronominal roots *ta* and *ra*. (Cf. also KUHN in his *Zeitschrift*, 14, page 229.) SCHERER, on the other hand, took up arms for the predicative roots, and was in favor of granting much wider scope to this kind of derivation than BOPP did, so that he considers it possible, for example, to connect the suffix *va* with the root *av* "satisfy one's self", "fill".

It seems a matter of course to the adherents of BOPP's agglutination theory that in attempting to explain the stem-

<sup>1)</sup> Previously (page 14) the Semitic family had been brought up for comparison.

forming suffixes recourse should be had to BOPP's two root-classes, or to one of them. I must, however, confess (in agreement with SCHERER) that I can only form a clear idea of the derivation of suffixes from predicative roots, since we have an excellent analogy before us in support of this derivation, in the shape of our German suffixes *-bar*, *-heit*, *-thum*. It is true, the assumption that pronouns are discernible in many suffixes is favored by the identity of form, or the similarity they bear to pronominal roots, but it is difficult to find the connection of meaning. We can say that the pronoun betokens the person or thing in general, which is afterwards more accurately defined by the predicative root to which the pronoun is affixed (so WINDISCH in CURTIUS' *Studien*, 2, page 402); or that the pronoun points, like an article, to the completed word (so CURTIUS in his *Chronologie*); but it must always appear strange that so many suffixes with almost the same meaning appear side by side, and that it is impossible to discover in these suffixes aught of the specific sense of the pronouns.

Under these circumstances, we cannot be surprised that attempts have been made to explain the stem-forming suffixes in a different manner, as by BENFEY, and also, with exclusive attention to certain forms of suffixes, by SCHERER and FICK.

BENFEY has expounded his theory in several different places: in his essays in the *Kieler Monatsschrift* of the year 1854; in his short Sanskrit Grammar; in various passages in his periodical, *Orient und Occident*; and in the briefest and clearest form, in an article in the ninth volume of *Kuhn's Zeitschrift*, entitled: *Ein Abschnitt aus meiner Vorlesung über vergleichende Grammatik der indogermanischen Sprachen*. The practical application of the theory can be seen most readily in LEO MEYER's *Vergleichende Grammatik der griechischen und lateinischen Sprache*, 2<sup>nd</sup> volume, Berlin, 1865.

This theory of BENFEY can be summed up as follows. The suffixes, which in the transmitted languages have such varying forms, were not different in the beginning; on the contrary, it is very probable that all, or almost all, must be derived from one fundamental form *ant*, which appears in the present active participle. But this *ant* itself is a metamor-

phosis of the third person plural in *anti*. Accordingly, from *bhārantī* "they carry" came *bharant-* "carrying", and from this, *bhara-* "carrier" etc. For the original *ant* has undergone a long succession of phonetic changes; *ant* by weakening became *at*, then was corrupted into *an*, and further into *a*; *at* was transformed into *as*, and *an* into *ar*; *a* was changed to *i*, and so stems in *it*, *in* and *is* arose; further, through affixion of "the pronominal theme *a*" were obtained *anta*, *ata*, *ana*, *ara*, *asa* and *isa*; and so on. Those suffixes, also, which begin with *v* or *m*, like *vant* and *mant*, probably belong with the above in respect to origin. For perhaps *vant* was derived from a third person plural *vanti*, which belongs to a perfect with *v*. But this perfect with *v* is compounded with *bhū* "be", and the *v* is the last remnant of *babhūva*. The suffix *mant*, on the other hand, is supposed to have sprung from *tmant*, which came from *tvant*; but *tvant* itself is perhaps a participle from *tu* "be strong". (Cf. BENFEY, *Kurze Sanskrit Grammatik*, § 336, remark, page 212.) This *tvant* then became differentiated in course of time, so that it developed into *tva* on the one hand and *māna* on the other.

If, now, all these assertions could be proved, and all, or almost all suffixes accordingly traced back to *ant*, which in turn comes from the third person plural, it would at the same time be demonstrated that all nouns are derived from verbs, and thus the hypothesis of the primary verbs, which was mentioned above (page 78), would be justified.

Against this theory, as we have just sketched it, we find the following weighty arguments:

First: It is impossible to see clearly how the participle could have arisen from the third person plural. It would be easier to comprehend how the reverse could take place. (V. below, under C. in the following section.)

Secondly: In the changes which the suffixes undergo, phonetic processes are assumed which cannot elsewhere be shown to occur. It is also a questionable assumption that one and the same form could have developed under like conditions into two wholly different shapes, as e. g. *tvant* into *tva* on the one hand and *māna* on the other.

Thirdly: If all nouns can be finally traced back to for-

mations with *ant*, we must assume that the suffixless nouns so frequent in the oldest Indian literature, like *divis*, *ud* etc., once possessed suffixes and then lost them (of course at a very ancient period). BENFEY does make this assumption, but so far as I see it can be supported by nothing except the very necessity of his system. In conclusion, it must be remarked that after all it is impossible to derive all suffixes from *ant*, and that BENFEY himself has to make occasional use of pronouns as one source of suffixes.<sup>1)</sup>

For the above reasons I cannot agree with BENFEY's view, but it is self-evident that in rejecting the hypothesis as a whole, we do not necessarily give up every derivation of one suffix from another. Whether such a derivation shall be assumed or not, must be especially considered in each individual instance.

SCHERER, whose general views regarding suffixes have already been mentioned, originated the hypothesis that a number of suffixes were really signs of the locative, that is, that the stems formed by them were locatives. Thus he observes in regard to the suffix *a*:

"Those who assert that *a* gives a substantial sense to the root, that it is the universal *it*, or in regard to persons the universal *he*, move in such a dizzy height of abstractions that I cannot follow them. All my ideas of language rebel against this. I regard the *a* of stem-formation as no other than the *a* of word-formation, i. e. declension. We know its locative meaning and prepositional application, which starts from the idea of union with anything. But what is the simplest and clearest way of denoting the possessor or accomplisher of an action, state or quality? What more so than to say that he is in this action, this state, this quality, he is united with them?" (*Zur Geschichte der deutschen Sprache*, 1<sup>st</sup> edition, page 331.)

I object to the above theory that the accomplisher of the action, the possessor or exerciser of the quality is really not expressed at all (for a *bhar-a* would thus mean "in carrying", but not "one who is engaged in carrying"); and above all, I

<sup>1)</sup> A detailed criticism of BENFEY's view, with which the above remarks are in harmony, is given by ZIMMER, *Die Nominalsuffixe a und ü* (Strasbourg, 1876).

would state that I am persuaded with KUHN (in his *Zeitschrift*, 18, page 365 seq.) that a locative suffix *a*, such as SCHERER assumes, cannot be shown to exist. Nor does it seem to me that SCHERER has in general made it appear probable that declension was prior to stem-formation, so that I am not prepared to accept the explanation that a stem-forming suffix was derived from a locative.

Finally, FICK (who must be mentioned third in this connection) disputed the entire existence of an *a*-suffix, in an essay in *Bezzenberger's Beiträge* (1, page 1 seq.). He starts with the assumption that those stems to which the suffix *a* was previously ascribed, are at bottom identical with certain present stems, as e. g. δόμος with the present stem δεμο- in δέμεμεν. Then, in accordance with the root-theory described above, he resolves these present stems in a different manner from the usual one, separating δεμο- not into δεμ-ο, but into δε-μο, Indo-European *da-ma*; and by adopting a similar division in every case, he becomes convinced that a normal stem-forming suffix *a* never existed. But this conclusion leads to the greatest difficulties. Consider for example the following: are we really to resolve the roots *av* "refresh", *as* "be", *an* "breathe", *am* "oppress", and a number of others of like formation, into *a-va* etc., assuming *a* as their foundation, and therefore as their simplest root-form? Under this supposition the oldest language could hardly be characterized as intelligible. If FICK's method were mathematically certain, it would be impossible to avoid adopting this extraordinary conclusion, as BEZZENBERGER does (*Gött. Gel. Anz.*, 1879, article 18, page 558); but as it is, the correctness of the method must be doubted, in consequence of a result so difficult to accept. So I cannot make up my mind to withhold the name of suffix from the element *a*; and we shall see below that the occurrence of *a* in tense-formation is also no sufficient ground for denying that it can possess the quality of a noun-suffix.

I must therefore acknowledge that none of the theories mentioned above is more to my taste than that of BOPP. Whether, indeed, it ever will be possible to attain to more than a certain degree of probability in this field, can be reasonably doubted.

I must remark particularly, in conclusion, that in the individual languages exactly the same is true of the reality of stems as of the reality of roots. Stems can have existed only in the primitive speech, before the development of cases. If, notwithstanding, we postulate noun-stems in Greek, Latin etc., this occurs merely from practical considerations.

### *B. Case-formation.*

If in our consideration of the Indo-European cases we use the analogy of the declension in the Finno-Tartaric languages, we easily arrive at two suppositions, which are also recommended by their naturalness, viz., the supposition that once, in the Indo-European itself, every case had only one and the same sign in all numbers; and that there was a general plural sign. But it is not possible, by means of the case-suffixes which actually exist in the Indo-European languages, to establish the correctness of these two suppositions, by which several scholars, consciously or unconsciously, have been influenced in their attempts to explain the case-suffixes. We not only find the most various signs for the same case in different numbers (e. g. *as* and *sya* in the genitive singular, and *ām* in the genitive plural), but there are also different signs for a case in one and the same number (e. g. in the locative singular); and SCHLEICHER, despite all his efforts, is by no means able to prove the former existence of the plural *s* in all the cases of the plural. On the other hand, it cannot be denied that there is something to be said in favor of the two above-mentioned hypotheses, and it is therefore natural to suppose that the original form of the Indo-European declension has been distorted almost beyond recognition. Reasons for such a distortion could be easily found. It is more than probable that the Indo-European originally possessed many more cases than those we now find in the noun-declension of the Sanskrit, and it is therefore possible that where we think we discover several endings of one and the same case, there were originally several cases, and that the endings are lost which would give us the missing parallels to those still preserved.



In such a hopeless state of affairs it does not seem expedient to examine the attempted explanations in detail; I will content myself with briefly indicating the two main tendencies which can be followed in the explanation. We can either assume that the case-suffixes were affixed in the beginning in order to denote something similar to the present cases, and that they contain pronominal, or pronominal and prepositional elements; or we can assume that stem-forming suffixes developed into case-suffixes, so that, for example, the genitive in *-sya* would be nothing but a stem used as an adjective. This latter opinion is adopted by CURTIUS for some cases, by ABEL BERGAIGNE (*Mém. de la soc. de linguistique*, 2, page 358 seq.) for the majority, by LUDWIG for all.

I cannot see what serious objection there can be to granting a certain amount of latitude to both theories (as CURTIUS does), but the uncertainty is here so great in every case, that after repeated consideration of the whole question (to which I have been constantly led by my syntactical labors), I have arrived at no other solution than an ever recurring *non liquet*.

### III. THE VERB.

In the present investigation the attempt of course will not be made to give a history of the origin of the verbal system, so that in this connection much of what has been discussed in CURTIUS' *Chronologie*, and recently in my *Grundlage der griechischen Syntax* (*Synt. Forschungen*, 4), can be passed over in silence. The question here is only this, how far the agglutination theory can be applied in the case of the verb. I shall therefore treat only of: A, the tense-stems; B, the mode-stems; and C, the personal endings.

#### A. The tense-stems.

Among the tense-stems we have first to consider the manifold form of the *present stem*.

Of the syllables which are characteristic of the present stems BOPP speaks in his *Conjugationssystem*, page 61, as follows: "In Greek, as in Sanskrit, certain accidental letters are appended to the roots, which are only retained in certain ten-

ses, and disappear in the remainder. We might, as in Sanskrit, make this the basis of classification into different conjugations, which would then mostly coincide, in their characteristics, with the Sanskrit ones."

What BOPP says in the Comparative Grammar, § 495, shows a great advance upon this former stand-point. The passage in question is as follows: "It is hardly possible to say anything positive concerning the origin of these syllables. It seems to me most probable that the majority are pronouns, by means of which the action or quality, which was expressed in the root in the abstract, becomes something concrete; for example, the expression of the notion 'love' becomes an expression for the person who loves. But this person is more strictly defined by the personal ending, whether it is 'I, 'thou' or 'he'."

Here we find an intimation of what BENFEY and KUHN afterwards announced with respect to the present stem with *nu*, viz., that this is really a noun-stem, and that therefore the present-stem *dhr̥ṣṇu* in *dhr̥ṣṇumás* "we dare" is nothing but the adjective *dhr̥ṣṇús* "bold". This explanation was then extended to other present stems, especially to those which end in *a*. According to this theory, we see in the *o/e* of *λέγο-μεν*, *λέγε-τε*; *φεύγο-μεν*, *φεύγε-τε* not a union-vowel [*Bindervocal*], which is interpolated for euphonic reasons, or which (as POTT assumed) represents the copula, but the noun-suffix *a* of which we have spoken above. Whether the same view shall hold good for all present stems is a question on which opinions differ. CURTIUS, for example, sees in the present sign *ya* the verb *yā* "go"; others the noun-suffix *ia*. (Cf. BRUGMAN, *Zur Geschichte der präsensstammbildenden Suffixe*, in the *Sprachwissenschaftliche Abhandlungen, hervorgegangen aus Georg Curtius' grammatischer Gesellschaft zu Leipzig*, Leipzig, printed by Hirzel in 1879.) At all events, according to this theory the great majority of present stems would be really noun-stems, with the personal endings appended to them in the same way as to roots, so that, for instance, the same element would exist in *ἄγο-μεν* as in *ἄγό-ς* "driver", and an original *agetī* would really mean: "he is driver".

FICK opposed this view in two articles in the first volume

of BEZZENBERGER's *Beiträge*, one of which has already been mentioned. He first establishes the fact that noun-stems and tense-stems often coincide overlooking, at that time, the difference of vocalism, such as exists between  $\delta\acute{o}\mu\omicron\varsigma$  and  $\delta\acute{\epsilon}\mu\omicron\mu\epsilon\nu$ , which must certainly be traced back to the parent speech, and concludes from this that it is unlawful in such cases to speak of especial noun-suffixes. Now from the simple fact of the identity of noun-stems and tense-stems it is impossible to draw this conclusion, for this identity may have arisen from the subsequent assimilation of the independently formed noun-stem to the tense-stem. But this identity is not FICK's only ground for his objection to certain nominal stem-forming suffixes: in addition to this, he seems to be influenced by the idea that the tense-stems were always prior to the others. I say "seems", because, so far as I can see, he has not expressed himself clearly on this point: yet we can find a number of indications which tend in that direction, as for example: " $\epsilon\pi\epsilon\varsigma$ ,  $\mu\acute{\alpha}\chi\eta$  and  $\beta\acute{o}\sigma\kappa\omicron\varsigma$  are nothing but the verbal forms used as nouns"; or: "the proof that the so-called nominal  $\alpha$ -stems are identical with verbal  $\alpha$ -stems", — in which clause it must be noticed that only the noun-stems, which FICK in general handles with a certain irony, receive the epithet "so-called". He further speaks of the "nominal shading of  $\epsilon$  into  $o$ " (page 14; he accordingly looks upon the vowel of the verb as original. If, now, the verb-stems are prior to the noun-stems, the question naturally arises, whence do these elements of the verb originate, which may not receive the name of suffix? For the suffix  $\alpha$  FICK made the previously mentioned attempt at an explanation (page 82), but for  $ia$  (which he treats in the second article) such an attempt is wanting. Accordingly, before we can pass definite judgment on FICK's actual theory, we must wait until he has perfected his system in this direction.

At the point which present investigation has reached, the affair seems to me to take the following shape. It is obvious that the prototypes of certain tense-stems and certain noun-stems are the same. Whether, now, we are to assume that these prototypes possessed a character which was neither verbal nor nominal, i. e. such a sense as we ascribe to roots (which is SCHLEICHER's opinion): or that they were originally nouns,

- which adapted themselves to the verbal system; or verbal
- stems which were used as nouns, — this is a question which
- each one must answer in accordance with the idea which he
- has formed of the development of Indo-European inflection.

I pass on to the aorist and future.

- As shown above, it was principally in consequence of a
- scholastic error regarding the three parts of speech that BOPP
- was led to his hypothesis that the root *as* inheres in the *s*-ao-
- rist and the future. The origin of the hypothesis cannot,
- therefore, be quoted in defence of its correctness. Let us now
- consider whether other reasons can be adduced in support
- of it.

BOPP finds such a ground in the circumstance that the *s* appears twice in one form of the Sanskrit aorist, e. g. in *áyā-siṣam* from *yā* "go", which did, indeed, favor the assumption that the *s* belonged to a verb. BRUGMAN (CURTIUS' *Studien*, 9, page 312) objects to this view, first, that it is difficult to see what purpose the reduplication can serve here, and secondly, that from the stand-point of the Sanskrit forms an easier and more natural explanation is offered. There are in Sanskrit the aorists *áyāsam áyūsīs áyāsīt*, and *ávedīṣam ávedīs ávedīt*. Is it not very natural that after the analogy of *ávedīṣam*, a first person *áyāsiṣam* should be formed to *áyūsīs*? I consider this supposition especially probable, because the existence of this aorist is only proved for the Sanskrit.<sup>1)</sup> I cannot, therefore, regard it as an established fact that the double *s* of *áyāsiṣam* has any weight in favor of BOPP's hypothesis.

On the other hand, it cannot be denied that BOPP's hypothesis possesses considerable intrinsic probability. For it is a very natural supposition that beside the direct inflection of a verb, the indirect one, formed by affixing forms of the auxiliary verb *as*, could also be employed. (Various views can meanwhile exist concerning the nature and significance of this composition; cf. CURTIUS, *Chronologie*, pages 55 and 64.)

This assumption cannot, indeed, be proved, and it is

<sup>1)</sup> BEZZENBERGER, *Beiträge*, 3, page 159, note, is of a different opinion. But cf. BRUGMAN's reply, *Morph. Unters.*, 3, page 83, note.

therefore not surprising that another has been brought forward, namely, by ASCOLI (cf. CURTIUS as quoted above, and *Kuhn's Zeitschrift*, 16, page 148), who is of opinion that the aorist stem, as well as the present stems (discussed on page 93), has, perhaps, a nominal character. But the aorist stem by no means furnishes so plausible a foundation for the hypothesis as the present stems, and accordingly this supposition seems to me improbable. The future is, in the main, subject to the same judgment as the *S*-aorist.

### B. The mode-stems.

JOHANNES SCHMIDT has demonstrated (v. *Kuhn's Zeitschrift*, 24, page 303 seq.) that the sign of the optative in Indo-European was *iā* and *ī*, with the distinction that *iā* occurs wherever the syllable has the main accent, and *ī* where this is not the case. Accordingly, we shall have to assume that *iā* is the original form of the mode-element, and *ī* a contraction of it. Can, now, this *iā* be considered identical with the Sanskrit verb *yā*? This view, which is on the whole that of BOPP, is opposed by a weighty objection (also emphasized by SCHMIDT) in respect to the meaning involved. The first person can be explained very well in this way, but not the second and third; thus it seems as if *hanyās*, under this supposition, can only mean "thou wishest to kill", and not what it in reality signifies, — "I wish thee to kill". The question arises, however, whether we shall allow the whole hypothesis to be shipwrecked on this difficulty. We could perhaps assume that the meaning of the first person influenced that of the second and third, or we could regard the future significance as the original one, and derive the wish from it. (Cf. *Synt. Forsch.*, 4, page 115 seq.) Postponing to another opportunity the further discussion of this difficult question, I will content myself with having here intimated the various possible methods of explanation.

In regard to the subjunctive, of which *a* is the well-known sign, SCHERER sees in the *a* of *hānati* "he shall kill" the same *a* in which he recognizes a locative suffix. Such an explanation would, it is true, correspond to the sense of the subjunctive (for *hana* would then mean "for killing"); but as

already remarked (p. 89), I cannot grant the existence of an Indo-European locative suffix *a*. SCHERER's attempt cannot, therefore, bring into disrepute the view of CURTIUS, that the subjunctive is in form nothing but an indicative; i. e. the subjunctive *hánati* has the same formation as the indicative *bhárati*. CURTIUS explains the meaning of such indicatives as originally *durative*, and tries to derive from this the notion of the subjunctive, on which point I have expressed my agreement with him in *Synt. Forsch.*, 1. I will now acknowledge, however, that there is no necessity of assuming such an intermediate stage of meaning, and I would therefore prefer not to make that the basis of a derivation of the subjunctive from the indicative; but the external similarity of forms like *hánati* and *bhárati* still seems to me a very strong ground for assuming their original identity. I am inclined to agree with CURTIUS in regarding the subjunctive with *ā* as a sort of formation by analogy. In the distribution of meaning to the different persons the same difficulty exists in the subjunctive as in the optative.

### C. *The personal endings.*

I have previously (page 71 seq.) characterized as probable the assumption that pronouns inhere in the personal endings of the verb, and shall not at present return to the arguments for and against agglutination; I will simply bring forward what seems worth discussing within the bounds of the theory itself.

In the first place, it must be remarked that not all the scholars who consider affixion as on the whole probable are willing to accept it in the case of all the persons. There is a division especially in regard to the explanation of the third person plural of the active. The resemblance between the present active participle and this person is so conspicuous that there is a strong inclination to seek for some genetic relation between the two forms. BENFEY made this attempt by deriving *-ant* from *-anti*. I have already (page 88) declared that I am not of his opinion. ASCOLI and BRUGMAN have adopted the opposite method; the latter says (*Morpholog. Unters.*, 1, page 137):

"Who knows that *bháranti* is not the stem of the participle (*bhárant*), which our Indo-European forefathers used as third person plural, and to which later, although still in the period of the primitive speech, they affixed *-i* after the analogy of *bhárati*?"

It is difficult to decide whether the greater probability lies on the side of this view or that of POTT (i. e. that two pronominal stems, *na* and *ta*, are contained in the ending *-nti*), leaving BOPP's theory wholly out of account, according to which *n* indicates the plural in a symbolic way. SCHERER goes farther than the above scholars, and considers that the third person singular is also of nominal origin, i. e. that it is the locative of a participle. But there is no participle which stands in so close a relation to the third person singular as the present active participle does to the third person plural, so that the customary explanation seems to me the most natural, according to which the stem *ta* (which adapted itself to *mí* and *sí* in respect to form, as well as in its lack of distinction in gender) inheres in the suffix *ti*. (Cf. also KUHN in his *Zeitschrift*, 18, page 402 seq.)

It therefore seems probable that the three endings of the singular and the first two of the plural (the dual we leave out of the discussion) must be regarded as pronominal roots (which combine with the verb in a more general sense than could be expressed by one of the later cases), while the possibility must be held in reserve that the third person plural was originally nominal (like the Latin *amamini*), being subsequently added to the system of endings, and assimilated to the other forms.<sup>1)</sup>

All suppositions respecting the processes of composition, change and mutilation which the personal endings probably underwent in the parent language are amenable to grave objections. If we assume — to give merely one example — that *sí* was derived from *tva*, there is no proof that this cannot have taken place; but neither can any analogous process in the primitive speech be quoted in support of this assumption, which

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<sup>1)</sup> This supposition is made in regard to the imperative suffix *-tāt*, which was explained as an ablative, first by SCHERER, and afterwards by BRUGMAN (*Morph. Unters.*, 1, page 163). Yet the transition from the ablative meaning to the imperative is difficult to find.

rests simply upon the intrinsic probability of the supposition that all the suffixes of the second person belong to one stem. Now this probability is not so overwhelmingly great as to exclude all doubt. For why, BRUGMAN asks (*Morph. Unters.*, 1, page 135), would it not be just as possible to assume two stems for the pronoun of the second person, as for the pronoun of the first person, where the attempt would certainly not be made to trace back forms like *nas* and *vayám* to the same stem-form?

Equally unsatisfactory is the explanation of the middle endings by means of the double affixion of pronouns. It is true that their connection with the active endings is indubitable, but the method of development of the separate middle forms can hardly be established with certainty. The following difficulty must be especially considered. SCHLEICHER and CURTIUS explain the separate forms independently, assuming that the process of composition and mutilation has taken place in the case of each one. But is it not quite as natural to assume that the like endings are partly due to a process of borrowing? The other theory, which discovers a vowel-strengthening in the *ai* of the middle, cannot command our unqualified approval. I must accordingly hold to the opinion expressed in the *Synt. Forsch.*, 4, page 69, viz.: that none of the proffered explanations is secure enough for us to be able to erect hypothetical structures, syntactical or otherwise, upon it.

And the same is true with regard to the other questions which come up in this connection. In each separate instance we seem to find that the means at our command are not sufficient to enable us to choose with certainty between the different possibilities of development which are open to us. We must also bear in mind that the forms which we deduce by comparing the individual Indo-European languages have a long period of development behind them, a development which has perhaps so metamorphosed the forms in question as to render it impossible to recognize their original character.



We have already found in our discussion of the notion "root" that there are two periods to be distinguished in the history of the Indo-European, viz., the pre-inflectional or root-period, and the inflectional period. BOPP, it is true, did not express this idea in direct terms, and POTT even rejected it (although inconsistently, as we have seen), but we have shown above (page 76) that it is the inevitable consequence of BOPP's analyses. The inflection itself, however, cannot have attained its completeness in a moment, but must have developed by different stages, and hence the inflectional period must be subdivided. Credit is above all due to CURTIUS for having, in his *Chronologie*, brought into especial prominence the idea that in the development of language, just as in the stratification of rocks, different layers must be recognized.

But it is another question whether he (or any other, as SCHERER, for example) has succeeded in defining with any degree of probability the periods through which the formation of Indo-European inflection has actually passed. As may be inferred from the opinions expressed in this chapter, I do not feel myself in a position to discuss this question. Every hypothetical structure presupposes the existence of a number of single hypotheses, which may be regarded in themselves as securely established, and can then serve as support for the less certain ones. Now after having adopted a more or less skeptical stand-point in regard to each of the individual form-analyses, I must draw the conclusion that no structure can be reared on such a foundation. I must therefore confine myself to the assertion that inflection undoubtedly developed gradually, and not instantaneously, while I question whether the material we possess is sufficient to enable us to define the periods of its development.

The affair would, indeed, present a different aspect if we were in condition to amass new material; and Ascoli has made this attempt. This distinguished philologist, who is at home in the Semitic as well as Indo-European field, assumes that the Indo-European and Semitic parent speech were derived from a common source, and that they even possess certain noun-stems and the rudiments of declension in common. Were this assumption correct, it would prove that the inflec-

tion of the Indo-European began with the formation of noun-stems. I have too little familiarity with the Semitic field to pass judgment on ASCOLI's reasoning, and must therefore, to my regret, content myself with referring the reader to ASCOLI's own demonstration (most accessible in *Kritische Studien*, page 21).

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Having concluded our special discussion, we will now turn back to the beginning of this chapter, and inquire: has the agglutination theory been verified in individual cases? I can scarcely believe that the patient reader, who has followed me through the whole of the above demonstration, will reply with a confident "yes". For in the individual analyses a certain probability, at best, and not infrequently an empty "*non liquet*" has been the result. Accordingly, at the end of a long and toilsome pilgrimage we find ourselves no nearer the goal. Even now we cannot go beyond our previous assertion, that the principle of agglutination is the only one which furnishes an intelligible explanation of the forms.

There is nothing else we have met with which deserves the name of principle, certainly not the so-called "*symbolical*" explanation, in which BORP in some cases takes refuge, and for which PORT exhibits a still greater partiality. I do not feel competent to consider this method of explanation more in detail at present. For so far as I can see, it is so subjective that a discussion *pro* and *con* cannot be instituted.

Since, now, after our whole discussion the principle of agglutination is all that survives, the question arises whether it would not be better to relinquish philological metaphysics altogether, and confine ourselves to what can be really known; that is, whether we shall not define as the task of Indo-European philology the deduction of the fundamental forms (in SCHLEICHER's sense), and the explanation of the individual forms from these. As we saw above, JOHANNES SCHMIDT has expressed an opinion which tends in this direction, and many philologists certainly agree with him.

But I do not believe that this view will become general. The attempts to analyze the parts of speech do not, after all,

depend upon the arbitrary decisions and fancies of scholars, but are founded upon certain linguistic facts (as, for example, the resemblance of the personal and stem-forming suffixes to certain pronouns, and the like), and therefore will probably be repeated in future. Whether, indeed, in after times a more satisfactory result will be attained, it is not the province of the present to decide.

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## CHAPTER VI.

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### PHONETIC LAWS.<sup>1)</sup>

After having briefly shown in Part First how the notion of phonetic law has been constantly increasing in importance in the field of philology, I now pass to its discussion, which I shall handle in the following manner: I shall first explain the stand-point of GEORG CURTIUS, and then append my own treatment of the subject. I do not aim to say anything new in this discussion, but will merely strive to give a brief though comprehensive outline of what has been said by others.

In order to fully appreciate the stand-point of G. CURTIUS, we must remember (what may be easily forgotten now-a-days, when CURTIUS' principles are often opposed as being too lax) that his especial endeavor was to prove that a more rigorous order exists in the realm of sounds than his predecessors had succeeded in establishing, and thus to place etymology on a surer foundation. In the *Grundzüge*, 5<sup>th</sup> edition, page 80 [English translation, 1, page 104], he says:

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<sup>1</sup> The more recent literature on this subject may be found in the detailed and instructive essay of MISTELI on "Phonetic Laws and Analogy" (*Lautgesetz und Analogie*), in LAZARUS & STEINTHAL's *Zeitschrift für Völkerpsychologie*, 11, page 365 seq. In the following pages not only what is quoted by MISTELI, but also the essays of BENFEY and his adherents, on the same topic, are especially taken into account.

"If there really had occurred in the history of language such very sporadic variations and completely diseased and unaccountable corruptions of sound as are confidently assumed by many scholars, we should be obliged to renounce etymologising altogether. For it is only what is regular, and internally coherent, that can be scientifically investigated; what is arbitrary can at most be guessed at, never decided with certainty. The case is however, I believe, not quite so bad as that", but (page 81) "it is precisely in the life of sounds that fixed laws may be most surely discovered, which act almost with the consistency of the forces of nature".

CURTIVS, therefore, although he does distinguish an irregular or sporadic substitution of sounds [*Lautvertretung*], in opposition to the regular one, would by no means assert that phonetic change is in part exempt from all laws, and given over to chance and arbitrary action. "It is needless to say", as he remarks farther on (page 90), "that we do not regard either the one or the other class of phonetic change as accidental, but rather start with the opinion that laws penetrate this phonetic side of the language, as they do the whole."

How it is possible, despite this regularity, which prevails throughout language, for corruptions and abnormal changes to occur in the substitution of sounds, will become clear to us if we consider more closely the nature of this regularity.

In the first place, CURTIVS sees in all phonetic movement a pervading tendency or inclination. The fundamental tendency of phonetic change is a descending, diminishing one, or as CURTIVS prefers to call it, "disintegration" [*Verwitterung*]. "For in fact it is very natural to make a comparison with the stones, which are gradually diminished and wasted by atmospheric influences, yet in spite of this retain their core so persistently." (Page 409 [trans., 2, page 4].) Of course in the case of sounds the cause of the diminution does not lie in the action of external forces, but depends upon human convenience, which ever strives to make the pronunciation easier and easier. "Convenience is and remains the chief factor in phonetic change under all circumstances." (Page 23, note.) But this convenience displays itself chiefly in two tendencies. First, there is an inclination to exchange the less convenient

place of articulation for the more convenient, and therefore, since the place of articulation is more inconvenient the farther back it is situated, the inclination to form the sounds farther forward in the mouth can be established as a general tendency in phonetic change. So *p* arises in place of *k*, but not *k* in place of *p*. Secondly, the sound which in its nature is more difficult to pronounce is replaced by the more easily pronounceable one, and accordingly the so-called "explosive sounds" [*Explosivlaute*] pass over into the "fricative sounds" [*Fricativlaute*], while the opposite process does not take place. So *t* becomes *s*, but *s* does not become *t*. All phonetic change, even the sporadic, comes under these chief norms, whose validity CURTIUS tries to establish in special instances. "Even in the case of sporadic substitution of sounds, we must be guided by the principle that only a transition of the stronger sound into the weaker may be expected, and not the reverse." (Page 437.)

Phonetic change, therefore, may not overstep the general conditions imposed by these norms, although we must allow it a certain freedom of motion within these limits. Thus the original *a* of the European languages is represented, now by *a*, now by *e* or *o*, without definite discoverable grounds for this change of coloring; the Indo-European guttural *tennis* appears in Greek now as *x*, now as *κ*, now as *τ*, also without the possibility of adducing satisfactory reasons for this divergence; and in addition to these irregularities, which can always be brought into some system or ranged under some category, there are isolated abnormities, as for example, when an *s* at the beginning of a word regularly falls out in Greek, but a *σς* is preserved by the side of *ςς*, and many similar phenomena, with which every one is acquainted through practical experience.

CURTIUS by no means regards as wholly inexplicable this great mass of isolated exceptions, irregularities, corruptions and arrested forms, however he may christen them; he seeks, indeed, to discover the forces which can interrupt the normal course of phonetic change. Of such forces he mentions two: effort to preserve the significant sounds or syllables, and analogy. The first point he has especially treated in his remarks on the range of phonetic laws, particularly in Greek

and Latin (*Ber. der phil.-hist. Classe der Königl. sächs. Ges. der Wissenschaften*, 1870, July 1). CURTIUS tries to show in this essay that sounds and syllables which are felt to contain the chief significance oppose disintegration longer than others, and that accordingly the importance of the sounds must not be neglected in our judgment of phonetic change. What he says about the *ι* of the optative may serve as an example :

"The Greeks in general had a strong inclination to drop the last sound of the diphthongs ending in *ι*, before vowels ; hence we find *αω*, *εω*, *οω* for the older *ayāmi*, *ποιέω* frequently for *ποιέω*, etc. They followed the same tendency in the genitive singular, where at an early period *οιο* was contracted to *οο* and further to *ου*, Doric and Aeolic *ω*. On the other hand, the *οι* remained undisturbed in optative forms like *δοίην*, *λέγοιεν*, *γενοίατο*, *ποιοίην*. *Λαχόην* = *λάχοιμι* is only transmitted to us as an Aeolic form (AHRENS, page 133). It was evidently more necessary to save the mode-sign than the *ι* of the genitive. The latter case could still be plainly recognized without *ι*, even after contraction had taken place, while the optative formations would be almost unrecognizable without this *ι*, or at all events very unlike the other forms of the mode", etc.

The second point, analogy, has not been comprehensively treated by CURTIUS, but like other philologists, he has occasionally employed analogy as a principle in explanation. He by no means fails to observe that an important influence is exerted upon the whole theory of language by the conception which is formed of the working of analogy. In this connection, a sentence of the article mentioned above (of the year 1870), page 2, is of especial interest :

"Two fundamental notions are of the highest importance for linguistic research, that of analogy, and that of phonetic laws. I think I can hardly be mistaken in asserting that the difference of opinion which exists concerning individual questions depends in large measure upon the latitude allowed to each of these notions in the life of language."

By means of these two mental forces, i. e. perception of the significant value of a sound, and of the power of analogy, it is possible to explain many of the existing deviations, although by no means all. According to CURTIUS' view, quite a

large number remain, and I will call particular attention to one point, which seems to me of prime importance. CURTIUS not seldom assumes that from one and the same sound, or one and the same group of sounds, different results can arise under precisely similar circumstances. The declension of the comparative affords an example. From the  $\mu\epsilon\iota\zeta\omicron\nu\sigma\omicron\varsigma$ , which we must presuppose, could arise either  $\mu\epsilon\iota\zeta\omicron\sigma\omicron\varsigma$ , with retention of the  $\sigma$ , and from this  $\mu\epsilon\iota\zeta\omicron\upsilon\varsigma$ ; or  $\mu\epsilon\iota\zeta\omicron\nu\omicron\varsigma$  (perhaps through the intermediate form  $\mu\epsilon\iota\zeta\omicron\nu\nu\omicron\varsigma$ ), with retention of the  $\nu$ . (Cf. *Erläuterungen zu Curtius' griech. Schulgr.*, page 68.) It seems to me that such "doublets" (as BRÉAL calls them, in *Mém. de la soc. de linguistique de Paris*, 1, page 162 seq.) can only be explained by CURTIUS under the supposition that the speakers chose freely, and of course unconsciously, between existing possibilities. They are of especial importance in forming a conception of the phonetic laws, as we shall see later.

In this system of CURTIUS, which we have roughly outlined above, although not accurately portrayed, three notions are especially prominent: phonetic laws, analogy, and the preservation of sound on account of sense. I will discuss these three notions in inverse order.

In regard to the last point, the *influence of sense on sound*, I cannot convince myself that CURTIUS' view is the correct one. There is an objection to it on general grounds. It seems to me we are not justified in assuming that the Hindus and Greeks had a perception, which we have ceased to possess, of the significance of the individual sound in a linguistic form. For they, as well as we, had only completed words, which were transmitted to them from generation to generation; and that primeval period in which, according to the BOPPIAN assumption, the Indo-European forms were constructed, by the composition of significant elements, lay for them, no less than for us, in the twilight of the past, whence no enlightening ray could reach them. It also seems as if, in single points, more plausible explanations might be found for several of the phenomena discussed by CURTIUS. Thus in my opinion the preservation of the  $\iota$  in the optative, referred to above, may be more correctly regarded as due to the in-

fluence of analogy. It seems, in fact, the most natural assumption that δοίην remained (i. e. did not become δόην) because it formed part of a series δοῖμεν, δοῖτε etc. The case is the same with the *is* of the genitive in *noctis* (cf. CURTIUS in the above article, page 22), which was retained in consequence of the innumerable *is*'s in the genitive, while no similar analogy prevailed to an equal extent in the nominative; the same is also true with regard to the ι of φύλαξι, which was more protected than that of ἐνί, etc. Different explanations may be found for other points introduced by CURTIUS; thus, as he himself intimates, in explaining the different forms of prepositions, we must take into account the difference of accent, according as they are or are not used as proclitics. In this case, since the accent is a very important factor in the phonetic aspect of a word, we must seek the explanation on the phonetic side. Of course I am unable to solve many of the unexplained difficulties which CURTIUS brings forward in the article mentioned above; but I can at least assert that no conclusive proof has yet been given that those sounds which were felt to contain the chief significance were occasionally preserved, in direct opposition to prevalent phonetic laws.

I am therefore of the opinion that we have not yet the right to admit this idea into the *répertoire* of philology.

*Analogy*, the second of the notions emphasized by CURTIUS, has already been mentioned in its historical development. I will here repeat that this principle was not ignored in earlier times<sup>1)</sup>, but that lately it has been much more frequently applied, owing to various causes, among which are the example of modern tongues, the conviction that the new formations of individual languages depend upon imitative formation, and above all, the attempt to establish exceptionless rules in the case of phonetic change. The question arises

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<sup>1)</sup> In MISTELI's article, BENFEY might have been quoted, as well as POTT and CURTIUS, since as early as the year 1865, in the *Orient und Occident*, 3, page 225, he spoke as follows regarding the Vedic language: "It is not without a purpose that throughout this essay I have called attention to the examples of false analogy, by which the Vedic language is forced into the most diverse channels."



whether and in what way this frequency of application can be justified, and whether it is possible to set certain limits to the employment of the principle of analogy, and within these to hold fast to certain distinctions and divisions.

In regard to the first point, the demarcation of the field, so far as I can see, no practical directions have hitherto been given. It is true that Misteli in the article quoted above, page 410, laid down the principle that not too many and not too complicated workings of analogy must be assumed for the individual instance: but this general direction is no help in the individual instance, since in each case the question "what is many?" and "what is complicated?" will find various answers.

Another suggestion seems more plausible at the first glance. It is natural to assume that the forms which exercise the attractive force (i. e. produce the analogy) must be more numerous than the attracted ones. But on closer examination this reasoning is found to be invalid. In my opinion, at least, BRUGMAN is right when he argues (in *Kuhn's Zeitschrift*, 24, page 50, and *Morph. Unters.*, 1, page 82 seq.) that the action of analogy takes place gradually, that is, one form may attract a second to it, these two a third, fourth and fifth, and these further the following ones, up to the thousandth, etc., so that we can easily imagine that a mere handful of forms may have served as the model for thousands. Such cases do actually occur; thus, BRUGMAN adduces the fact, already established by other scholars, that "four Old Slavonic verbs, *jesmĭ*, *vĕmĭ*, *damĭ* and *jamĭ*, have brought it to pass that in New Slovenian and New Servian the verbs of all the conjugational classes end in *-m* in the first person singular", — and similar examples. (Cf. *Morph. Untersuch.*, 1, page 83.)

It seems to me, therefore, that hardly any practical suggestions have been offered in regard to the boundaries within which the action of analogy takes place.

Perhaps it would be easier to say something of the various kinds of formation by analogy. Since a formation by analogy is a change of form which occurs in consequence of an association of ideas, we can make a classification from three following points of view: from the *nature of the psychic processes* which play a part in such a formation: from the *constitution*

of the words in question; and from the result attained by the action of analogy. I will discuss these three points briefly in the above order.

First, in regard to the classification according to the *psychic processes*: much that MISTELI has brought forward on this subject may serve to introduce its discussion, which has not yet begun in earnest. I will only emphasize one point here: it is important to distinguish whether a transfer of form has taken place of itself, so to speak (as is the case in the greater majority of instances), or whether the speaker, finding the form which is demanded by the phonetic laws for some reason inconvenient, seeks for some other formation, and as the result of this search a transfer of form takes place. An example of the latter sort is the Latin dative and ablative plural in *-abus*, which frequently occurs in *deabus*, *filiabus* and *libertabus*, and in isolated instances in other words. As is most clearly shown by the passages in NEUE'S *Formenlehre der lateinischen Sprache*, 2<sup>nd</sup> edition, 1, page 22, these dative-ablative forms arose where a distinction from the corresponding forms of the masculine was needed. There was no objection to saying *di deaeque*, *deorum dearumque*, *deos deasque*; but what should be said in the dative and ablative, — *dis disque*? There was a similar drawback to the use of *filia* in wills or other provisions *quae pertinent ad necessitatem juris*. Suppose, for example, provision must be made in case a son or sons, a daughter or daughters are living. Should it be worded: "*filio seu filiis, filia seu filiis exstantibus*"? It was evidently in such and similar predicaments that the forms in *-abus* originated, and the process can hardly have been other than the following: another form is sought instead of *dis* and *filiis*, which in special cases could not be employed; and this form is suggested in consequence of the connection subsisting in the mind of the speaker between the series *filiae, filiarum, filiis, filias*, and *duae, duarum, duabus, duas*. In ordinary speech the ablative *duabus* can exercise no attractive force on *mensis* and the rest, because their *is* is protected by connection with the *is* of the second declension, which has the same significance. Not until this connection is for some special reason dissolved, does *duabus* exert its attractive force. The old grammarians are

therefore quite correct in saying that the forms *deabus* etc. were created *differentiae causa*; but the impulse toward differentiation was not able to evolve new and original formations, only imitative ones, after existing models. This impulse to differentiate can accordingly be classed among the motives which are active in the construction of forms by analogy. (Cf. MISTELI as quoted above, page 472.)

We find a second ground of classification in the *constitution of the words in question*, that is, in the conditions which must be present in the words before any action of analogy can take place. Under this head we must ask first of all whether words connected only by sound, and also whether words connected only by sense, can influence each other through the working of analogy. I should be inclined to answer the first question in the negative, the second in the affirmative. To illustrate the first, Misteli gives a good example (page 434), which I will repeat here:

"Although  $\kappa\alpha\theta\acute{\iota}\zeta\omega$ ,  $\acute{\epsilon}\kappa\acute{\alpha}\theta\iota\sigma\alpha$  forms in the future  $\kappa\alpha\theta\iota\tilde{\omega}$ ,  $-\theta\iota\sigma\acute{\iota}\varsigma$ ,  $-\theta\iota\sigma\tilde{\iota}$ , as if  $\kappa\alpha\theta$  were the root and  $\iota\zeta\omega$  the ending, as in  $\beta\alpha\delta\acute{\iota}\zeta\omega$ ,  $\beta\alpha\delta\iota\omicron\upsilon\mu\alpha\iota$ , so that scarcely a shadow (in the  $\iota$ ) of the root *sed* remains; yet despite the identity of ending,  $\kappa\acute{\alpha}\theta\iota\zeta\epsilon$ ,  $\kappa\alpha\theta\iota\zeta\omicron\nu$ ,  $-\theta\acute{\iota}\zeta\omega$ ,  $-\theta\acute{\iota}\zeta\omega\nu$ ,  $-\theta\acute{\iota}\zeta\omicron\iota\varsigma$  have not the remotest connection with, for example, a  $\pi\rho\acute{o}\rho\rho\iota\zeta\epsilon$ ,  $\pi\rho\acute{o}\rho\rho\iota\zeta\omicron\nu$ ,  $-\rho\acute{\iota}\zeta\omega$ ,  $-\rho\acute{\iota}\zeta\omega\nu$ ,  $-\rho\acute{\iota}\zeta\omicron\iota\varsigma$ ; the gulf between noun and verb cannot be bridged over by any amount of phonetic identity, and it is only because we regard this as self-evident that we can speak of purely phonetic analogy."

As to the second point, it is at least clear that endings whose function is identical enter into association, even without phonetic similarity; thus,  $\acute{\alpha}\gamma\omega\nu\omicron\iota\varsigma$  arises from analogy with the dative plural in  $-\omicron\iota\varsigma$ , while there is no seductive similarity of form between  $\sigma\iota$  (in  $\acute{\alpha}\gamma\omega\sigma\iota$ ) and  $\omicron\iota\varsigma$ . Whether the same can be observed in word-stems (e. g. whether the form of the adjective "good" can influence the form of the adjective "bad", or the like) must be more accurately investigated. CAROLINA MICHAELIS (*Studien zur romanischen Wortschöpfung*, page 35) assumes such an attractive force in the case of the Italian *greve*, which would accordingly owe its *e* to the influence of the *e* of *leve*. In the second place, we must remark that in

inflected words the associative action can start either from the word-stem or from the endings, and in this connection a distinction must be drawn between *material* and *formal* analogical construction.<sup>1)</sup> An example of *material* formation by analogy is the Greek ἡδέσι, which came from the previously existing form ἡδύσι through the influence of ἡδέος, ἡδέε, ἡδέων. In the singular the forms ἡδός, ἡδύ, ἡδύν were able to resist the attractive force (although ἡδέα does occasionally appear); but in the plural, where, after the assimilation of the accusative to the nominative, ἡδύσι was the only case with υ, that constituent part of the forms (all belonging to one series) which was felt to contain the chief significance was made uniform. The innumerable *formal* constructions by analogy are illustrated by forms like ἀγώνοις, *Herzens* etc.

A third ground of classification is found in the *result of the transfer of form*, according as the original form is wholly supplanted by the imitative formation, as is the case with ἐλύσαμεν, which probably took the place of an older \*ἔλυσαμεν; or both forms exist side by side, as in the genitive *senatus* and *senati*. The question also comes up, whether an intermediate form can arise through the mutual influence of two forms, a species of formation which has received the name of "formation by contamination" [*Contaminationsbildung*]. An example would be the Latin *jecinoris* by the side of *jecur*. As the Sanskrit shows, where the stems *yakan* and *yakrt* exist side by side, the Latin paradigm was once *jecor*, \**jecinis*, and *jecinoris* is "contaminated" from both stem-forms.

But these and similar attempts, which may possibly be made, to classify the whole mass of analogical formations, cannot possess any considerable value for the practical application of the science, since the first task must be to collect material systematically in the newly explored field. I think such a collection would have the greatest likelihood of success if the inflectional forms of a definite linguistic period could be taken up, and the inquiry instituted, in what analogical formations each individual form was either actively or passively involved. This would be the easiest way to obtain a comprehensive view

<sup>1)</sup> OSTHOFF in particular (following PAUL) called attention to this classification in the lecture we shall mention below.

of the different series of forms which exist, or once existed, in the consciousness of the speakers. We should then find that all the cases of a word taken together form a series (from which fact we can explain, among other things, the leveling processes [*Ausgleichungen*] which take place between strong and weak cases), and also the corresponding cases in several (although not in all) subdivisions of declension, as we can see from the transfer of the locative *au* of the *u*-stems to the *i*-stems in Sanskrit (*kavai* formed after the analogy of *bhānai*). We should also find that nouns which belong together in meaning are so firmly welded into a group that occasionally even their cases undergo a leveling process. Thus the case-ending in *ur* (or *us*) of *pāti* "husband", *jāni* "wife", *sākhi* "friend", i. e. *pātyur*, *jānyur*, *sākhyur*, has certainly followed the genitive of the nouns of relationship, like *pitūr*. (Cf. WACKERNAGEL, *Kuhn's Zeitschrift*, 25, page 289.)

In the verb we should find these interchanges [*Verschlingungen*] in still greater abundance. We very soon see that not only the forms of one mode constitute a series, — e. g. *πεποιθαμεν* formed from *πέποιθα* in place of \**πέπιθμεν*, and so with the corresponding form of different indicatives, e. g. *ἔλυσε*, whose *ε* was derived from *ἔφερε*<sup>1)</sup> (cf. MISTELI as quoted above, page 436), — but that also the tense-systems of the individual verb influence each other in such a way that differences which have arisen phonetically, and which are so great that they threaten to break up the system of the whole verb, are adjusted; thus the old *δέλλω*, *ἔβαλον* has been supplanted by *βάλλω*, *ἔβαλον*; cf. JOHANNES SCHMIDT, *K. Z.*, 25, page 153. In the same way we find that the corresponding tense-systems of different verbs are connected in the mind of the speaker; hence e. g. the lengthening of the *i* in aorists like *ápīpatat*, which very probably arose after the model of *ácikradat*, so that now the same rhythm exists in all the forms which belong together.<sup>2)</sup>

<sup>1)</sup> BRUGMAN, *Morph. Unt.*, 1, page 161, derives this *ε* of the sigmatic aorist from the original *e* of the perfect, and following him, GUSTAV MEYER, *Gr. Grammatik*, page 402. [Transl.]

<sup>2)</sup> In accordance with the above, my former statement in the *Altindisches Verbum*, page 110, must be modified.

If such a consideration gives us an idea of the series and network of formations under which the word-forms are ranged in the mind of the speaker, we shall at once be able to formulate an important methodic principle (which has often been formulated; v. the passage in MISTELI's article, page 408), viz., the principle that the forms which stand outside the network of series, those, that is, which do not belong to the inflectional systems, have in their favor the probability that they will exhibit the unimpeded action of phonetic laws. At the same time, it must be clear from the few examples I have adduced that in all, even the oldest periods of the existing languages (and why not also in the Indo-European parent speech?), we may expect to find formations by analogy. It is true they will appear most frequently in more modern periods, because there a coincidence of the form-systems is favored by the more advanced mutilation of the endings. Yet we must again emphasize the fact that we are at present confined to general impressions and approximate estimates, since an exhaustive and classified material is not yet at hand.

I come to the third notion, — *phonetic laws*. In opposition to the view of CURTIUS, LESKIEN and others have, as we saw, brought forward a doctrine which can be most simply expressed as follows: *phonetic laws in themselves admit of no exceptions*. This phrase, which will be tested later, requires explanation. In the first place, it is self-evident that in order to become acquainted with regular phonetic development in its purity we must subtract all such results of analogical action as were described in the preceding section, and then we must consider that the natural development of sounds can be best studied in those languages which are as nearly as possible in a state of nature. The literary languages are less adapted to this aim, because they always possess a mass of borrowed words, borrowed either from foreign languages, or from related dialects, or from former periods of the same language, which are now only represented in literary monuments, — a borrowed mass, much of which has been so absorbed into the native material of the language that it is no longer felt by the speaker to be foreign. Which of us, even though he be a linguistic

scholar, would suspect, for example, that the word *echt* is a foreign word, which was received into the New High German literary language from the Low German? — and yet the fact does not admit of doubt. *Echt* is, as GRIMM expresses it, "a word unknown in all high dialects of the ancient language; even today the common people of Switzerland, Bavaria and Suabia are not familiar with it, and become acquainted with it only through the written language".

With the literary languages of antiquity the case is of course the same, or nearly the same, as with New High German, only we are more seldom in a position to prove that the borrowing has taken place, and must limit ourselves to suppositions. The Attic γένναϊος, for instance, with its double ν, is opposed to the phonetic condition of the Attic dialect, just as *echt* with its *cht* is to that of the New High German; would not the supposition, therefore, be justified (even although it cannot be historically proved) that the much-used γένναϊος was borrowed from an Aeolic dialect, just as the much-used *echt* was from a Low German one? The more words we find in a dialect which are liable to this suspicion of being borrowed from another linguistic mass, the more difficult it is to discover the original phonetic condition of this dialect. Now it is well known that it is precisely in the artificially developed Greek tongues that the borrowing of words and turns of expression plays an important part, and it is accordingly just here that there is danger of regarding what is foreign as native, and of thus assuming exceptions to the rule, whereas we really have phenomena which stand in no relation whatever to it. I desire to emphasize especially the latter idea, that the phenomena in the case of borrowed words stand in no relation whatever to the native rules of a language, because I find a misconception of this point on the part of CURTIUS, who remarks (*Grundz. der Etym.*, page 434):

"Another occasion for the disturbance of phonetic rules is furnished by the influence of the dialects upon each other. Disturbances of this sort are universally acknowledged, and cannot be wholly denied even by the most zealous defenders of regularity in this field".

It is certainly impossible to deny the fact that one dialect

borrowed words from another, but I cannot grant that a modification of the native rules is thereby effected. If an ethnographer finds some families of white immigrants in a land of dark-skinned inhabitants, he will not characterize the differing type of the former as an exception to that prevalent in the country, but his judgment will be that the whites must not be taken into consideration in a description of the aborigines; and the ethnographer, in my opinion, holds the same relation to those immigrants as the philologist does to foreign words, whether the latter are imported from the vicinity or from a distance.

In the case of a people who can read and write and have regular school instruction, the great amount which is borrowed from the literary language of an earlier stage of development, and brought into the speech of daily life, is of especial importance. It is in consequence of such a borrowing that in the cultivated High German language we vibrate between the dative "Mann" and "Manne", and the like.

When we have subtracted not only the results of analogical action, but also the whole mass of foreign words (in the broadest sense) which are present in a language, then, and not till then, can we deduce phonetic laws in their simple and unalloyed form.

Can it, now, be asserted that phonetic laws in this sense admit of no exceptions?

If in answering this question we first (as is only just) consult experience, we find that in the beginning the principles applied to Indo-European phonetics were tolerably lax, but that in the course of time (especially through the efforts of POTT, SCHLEICHER and CURTIUS) they became more and more rigid, and that we can observe a perpetual increase in the strictness of their practical application.<sup>1)</sup> Further, it cannot

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<sup>1)</sup> To show the progress which has taken place in the strict administration of phonetic laws in all departments of comparative linguistics, I will quote some remarks of two scholars who agree in their decided opposition to the new school of grammarians [*junggrammatische Schule*], — remarks of BEZZENBERGER und E. KUHN. BEZZENBERGER expresses himself as follows in a review of ASCOLI's *Kritische Studien* :

"On page 404, note 2, ASCOLI asks, in connection with the discussion in which he tries to ascribe to the 'original instrumental suffix -tra' a



be doubted that all scholars who have devoted any serious attention to phonetics have consciously or unconsciously been influenced by the idea that the moving spring of all changes is neither arbitrary nor accidental, but prevailingly regular. Yet on the other side the fact must be admitted that even in those fields where the work has gone on unceasingly for many years, much that is obscure still remains; and although it is to be hoped that more difficulties will be successfully overcome<sup>1)</sup>,

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progeny of somewhat surprising dimensions within the bounds of the Latin and Romanic languages: 'Or will FICK really assert that -θλο (-blo) is radically different from -bro? Will he, for example, separate \*φύθλα from the Hesychian φύτρα? Can we separate *latibulum* and *latebra*? I have not asked FICK what position he takes with regard to these questions, but I earnestly hope that he answers them all in the affirmative; and who could blame him if he should ask in turn: 'Can we identify *latibulum* with *latebra*? ASCOLI in my opinion is at fault when he says: '— a primary suffix, which would stand isolated, like a Greco-Italic -dhla'. I have already said elsewhere, and repeat it here, that the Slavonic -dlo- corresponds exactly to the Greek -θλο- and the Latin -bulo-. —, and if on the part of certain German scholars it has been preached, on the one hand, that phonetic laws admit of no exceptions, and on the other hand the Polish *radlo* is pronounced equivalent to the Greek ῥαδρον, this is merely one of the many instances of thoughtlessness exhibited by these very methodical 'investigators'." (*Gött. gel. Anz.*, 1879, article 18.)

E. KUHN's remarks are as follows (*K. Z.*, 25, page 327):

"What is the relation between Sanskrit *kumbha* and Zend *χumba*? The Aryan primitive form of both was *khumbha*; from this was derived without difficulty the Zend *χumba*, and it is well known that the Sanskrit *kumbha* has lost its first aspiration. The absence of aspiration in *stambh*, *stigh* etc. as opposed to *sthā* etc. is explained by the same rule. The whole question of the aspirated *tenues* needs a thorough revision, and now-a-days many who do not exactly advocate the fashionable folly of the infallibility of phonetic laws will find little probability in SCHLEICHER's assumption, according to which precisely the oldest examples of this phonetic class owe their origin to a wholly sporadic phonetic change."

<sup>1)</sup> A suggestive and valuable collection of such irregularities in the substitution of sounds, the cause of which is unknown, has been made by CURTIUS in his *Grundzüge*, 5th edition, page 429 seq. Whoever will reach these difficulties must, in accordance with the above, attempt it in three ways, by investigating:

1. Whether there is any borrowing. This is the case e. g. with *χιδναται* by the side of *σιδναται*, *τέγος* by the side of *στέγος*, and the like.

2. Whether there is any action of analogy. Under this head belongs e. g. the dative of the participle *λέγοντι*, which was prevented from becoming *λέγουσι* by its connection with *λέγοντος*, *λέγοντα* etc. The same is true of *χέρατι* and *παντί*. In *ἀντί* the τ was probably preserved because *ἀντ'* so often occurs.

no one can indulge in the delusion that it will ever be possible, in any language, to fully and entirely penetrate to phonetic change in its essence, and to view it in all its parts and developments. We are forced to make the confession: *it cannot be proved by induction that phonetic laws admit of no exceptions.*

We accordingly find ourselves compelled to seek a solution of the problem deductively, by considering from what causes and in what manner languages change, and above all, how it can be explained that different dialects arise from a homogeneous speech. In accomplishing this task we shall at the same time answer the question whether phonetic laws in themselves admit of exceptions or not. This point also I will discuss in connection with the theory of GEORG CURTIUS; yet I must first mention a mode of view which was formerly customary, but has now-a-days been almost entirely thrust into the background.

In the pre-BOPPIAN period it was customary to derive the difference of languages from the difference of the human vocal organs, and to explain this in great part from differences of climate. How often has a comparison been confidently made between the alleged harshness of the Doric dialect, with the wild, mountainous nature of the Laconian landscape, and the alleged softness of the Ionic, with the mild breezes of the coast district of Asia Minor! Whitney, in his *Language and the Study of Language*, pages 152, 153 spoke very decidedly against this old assumption, which, however, has lately been revived by OSTHOFF, who says:

"The formation of man's vocal organs, as well as that of all his physical organs, is especially dependent upon the conditions of climate and civilization under which he lives. Although it is generally known, for example, that the differing climate of a mountainous and a flat country causes a different development of lungs, breast and larynx in the inhabitants,

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3. Whether two sounds are concealed under one sign. This is probably the case with *f*; *c* with the consonantal *f* becomes *cc* (*c*), while *c* before the semi-vocalic *f* falls out. Only before the consonantal *f* does the so-called "prothetic" *ε* appear, as in *έεψα*.

It is to be hoped that through such treatment the lists drawn up by CURTIUS will be sensibly diminished.

yet it is a fact hitherto too little heeded in linguistic science, that everywhere under identical or similar conditions of climate and civilization, identical or similar phonetic tendencies are accustomed to manifest themselves in the language or dialect. I regret that I cannot here adduce sufficient examples to establish this principle. I will only remind the reader that in the Caucasus, for instance, neighboring nations, even when no original relationship exists between them, the Indo-European Armenians and Iranians, and the non-Indo-European Georgians and others, possess in the main an almost identical vowel and consonant system. It has been convincingly proved, above all by recent investigations in various fields, that within the limits of one and the same language an almost regular gradation prevails, or formerly prevailed, between the single dialects which constitute the common speech; for example, in the Germanic group, from the Alemannic of the Alps to the Low Saxon on the Baltic and North seas. I can hardly imagine that the regularity of climatic gradation covering the same area should not stand in some causal relation to this gradation of dialect." (*Das physiol. und psychol. Moment in der sprachlichen Formenbildung*, page 19, in the *Sammlung gemeinverständlicher wissenschaftlicher Vorträge*, herausgeg. von Rud. Virchow und Fr. von Holtzendorff, Heft 327.)

It is perhaps impossible to definitely answer the question whether climate and customs *also* have an effect upon phonetic change, which is all that OSTHOFF claims. It will certainly be granted in general terms that the climate cannot fail to have some influence upon the vocal organs, as it does upon the whole body; but on the other hand it must be confessed that physiologists have not observed such a difference of the organs as would explain the difference in the pronunciation of the separate sounds. The similarity which, according to OSTHOFF's statement, exists between neighboring languages, could perhaps also be explained by an influence exerted within historical times (for example, the Germans who live in Kurland have acquired something of the pronunciation of the Lettish people); and above all, the numerous changes of habitation made by the nations of every period are strong evidence against the theory. Should an influence of the cli-

mate upon phonetic change ever be demonstrated, a natural influence upon the formation of sounds would thereby be proved, which would then have to be distinguished from a social or historical one. I am not able to give a satisfactory answer to this question, which has thus been agitated anew by OSTHOFF, and will accordingly pass on to the theory of GEORG CURTIUS.

CURTIUS, as we remarked above (page 103), regards as the chief cause of phonetic change the attempt to make the task easier, the love of convenience which is characteristic of the human race, and WHITNEY agrees with him in the main. The latter scholar says in his *Language and the Study of Language*, page 70 :

"All articulate sounds are produced by an effort, by expenditure of muscular energy, in the lungs, throat and mouth. This effort, like every other which man makes, he has an instinctive disposition to seek relief from, to avoid: we may call it laziness, or we may call it economy; . . . ."

It is the result of this love of convenience, or this carelessness, that no generation speaks words precisely as they were spoken by the preceding one; only the fact that language is destined to be a medium of communication, and regard for its intelligibility (says WHITNEY), keep this carelessness within bounds.

The following considerations have especial weight against this theory.<sup>1)</sup> It seems to me very doubtful whether we have the right to assume that love of ease plays so predominant a part in human society. Would it not be possible to assert, on the other hand, that most men will exert themselves to imitate as accurately as possible what they have heard spoken, because they are afraid of making themselves ridiculous by deviating from the rest of mankind? — and further, that in speaking, not only what is convenient is aimed at, but quite as much what is pleasing? (cf. BENFEY, *Göttinger Nachrichten*, 1877, No. 21, page 550) — and that the impulse to consult convenience is opposed in a very effective and thorough man-

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<sup>1)</sup> See also LESKIEN in the *Jenaer Literaturzeitung*, 1875, No. 6.

ner by these and other conceivable motives? Perhaps still more weight must be given to an objection derived from practical experience; it was raised by ASCOLI, one of the most prominent masters of empiricism. ASCOLI asserts that in the languages which come under our observation, innumerable cases of phonetic transfer are found which cannot be explained from the principle of "weakening" or "making easier", as CURTIUS expresses it; and CURTIUS himself is by no means disposed to regard this objection as wholly unjustified (cf. *Grundzüge*, page 410), — indeed, in one important point he now assumes with ASCOLI a phonetic change opposed to the general principle to which he usually adheres, i. e. the change of  $\sigma\sigma$  into  $\tau\tau$  in Greek.

Under these circumstances it would be desirable to find a more general theory, in which, in addition to the desire for convenience, the other imaginable motives of change might find their place. This theory will be easily formed if we first ask the question, whether the changes which are here mentioned make their appearance all at once, among all the members of a community using a common language, or whether they start from an individual, or several individuals, and spread in different directions. It is only necessary to ask this question to answer it. If here, as well as in the whole of the following discussion, we disregard the possible influence of the climate, about which I can assert nothing definite, it is then clear that changes in pronunciation begin with the single individual, and are propagated by imitation throughout groups and masses. The final cause of all linguistic change, therefore, can only lie in the fact that the single individual does not circulate the language imparted to him precisely as he received it, but always *individualizes* what was transmitted to him, whether from love of convenience, or from an aesthetic impulse, or because his ear, in spite of every effort, could not accurately enough grasp it, and his mouth reproduce it, or from some other cause. Now the equalizing tendency of universal linguistic custom continually exercises a counter-check upon these innovations, so that change in the phonetic form of language is a result of these individualizing and equalizing forces. (Cf. especially BENFEY, as quoted above.)

The following will serve as further illustration of these general statements. We must be on our guard not to magnify the sphere of action of the individual (even leaving out of consideration the counter-influence of society). In the first place, we must consider that in the transfer of sounds practical interests hardly ever come into play, as may be the case in the transfer of words. It may happen that the chief of a warlike race suddenly issues the command that the appellations corresponding to certain ideas shall be changed, in order that the spies of the enemy may not understand the conversation of the warriors; or a prominent statesman or poet may for some reason bring forward a forgotten word and suddenly reinstate it in favor, — but in the field of sounds there seems to be no occasion for such a violent and arbitrary encroachment of the individual. Then we must not forget that the sounds of language (or a part of them) are arranged in series in the mind of the speaker, and that the change of one sound must inevitably induce a corresponding change of the remaining members of its series. If the pronunciation of *k* is changed in a certain way, the corresponding change of the remaining gutturals occurs spontaneously, and thus a considerable portion of the sounds are excluded from the possibility of an individualizing change. It would perhaps be advantageous if more weight were given to this idea, in our observations of phonetic change, than has hitherto been the case.

Finally, we must regard it as certain that all (or nearly all) these acts take place unconsciously. How true this assertion is with regard to our language of today we can easily convince ourselves by experiment. Most people do not know how they speak, and it often requires the greatest pains to convince them that they really possess certain fine shades of pronunciation which an experienced observer detects in their speech.

After the above remarks, we can comprehend the derivation of various languages from one, as well as the relative uniformity within the bounds of one language.

The first point, the derivation of various languages from one, demands no detailed consideration. If we imagine a little

community of men, say a hundred souls, who live together within a small territory, the impulses, proceeding from single individuals, to introduce innovations, will be readily and quickly counterbalanced by the habit and inclination of the remainder, and the process of leveling will take place without difficulty. If, now, we suppose a larger mass of men in a wider domain, still forming a community united for purposes of intercourse, the process will be a different one. The leveling will occur, but in each individual instance it will require more time than in the case of the smaller community, and there will always be marked differences between the separate natural groups of speakers, since some will still speak in the old way, while others employ the new. The leveling process will not come to a stand-still until there is a cessation of intercourse; the boundary of speech will then be formed in connection with the boundary of intercourse. Various historical complications may naturally occur in the formation of this boundary of intercourse. The following is a simple case. A tribe settles on the shore of a large stream, and subsequently a portion wanders over to the opposite shore. The intercourse naturally continues for a number of years, but gradually the ties which bind the wanderers to their old kinsmen become loosened, meetings take place only on rare occasions, and the linguistic impulses no longer cross the boundaries. Thus there is opportunity on both sides for the formation of a new language, which can develop more or less quickly according to circumstances. The case is more complicated if we assume that a portion of the emigrating party returns after a number of years; their language, if it has not yet gained a strong independent development, will perhaps be wholly absorbed by the old language; or it will retain its individuality by means of one or more peculiarities; or if the difference is already too great, a linguistic island will be formed, which may remain for centuries, until at length intercourse produces uniformity.<sup>1)</sup> But it is neither practicable nor necessary to bring forward in detail the endless variety of historical possibilities. In all

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<sup>1)</sup> This would be the place to mention the mixed languages [*Mischsprachen*], if a thorough treatment of them were in existence.

cases the principle will evidently be found true that no unity of speech can exist where there is no unity of intercourse.

It is more difficult to answer the question, how great the uniformity will be within the bounds of a homogeneous language. In the first place, it is clear that the different individuals of a linguistic community can never speak exactly alike; we must therefore confess at the outset that a homogeneous language in its strictest sense can only exist in the individual, or among a limited number of individuals, and the question which occupies us will accordingly be more accurately worded as follows: can it be expected, in the case of the single individual, that phonetic change will take place in a perfectly uniform and regular manner?

As we should expect, it is precisely in answering this question that the difference of stand-point becomes manifest. But in one respect perfect harmony seems to reign. So far as I see, it is universally admitted (or should be admitted) that in the passage from one pronunciation of a sound to another a *state of fluctuation* can arise, in which the same individual speaks now in one way, now in another. SIEVERS, for example (*Lautphysiologie*, page 127), says in regard to this point:

"The spontaneous construction of new phonetic forms naturally has its starting-point in the single individual, or a series of individuals, and it is only by subsequent imitation that these innovations are gradually transferred to the whole linguistic community to which these individuals belong. The complete adjustment between the colliding forms, the old and the new, may in some cases require a long time. For a certain space both forms will be used interchangeably; they will also be differently employed according to the position of the sound, until finally the new phonetic form wholly supplants the older." At the same time, SIEVERS mentions some instances of such fluctuation derived from practical observation: "Examples of fluctuation between two forms are found in many North German dialects, which use sonant and surd *mediae*<sup>1)</sup> without

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<sup>1)</sup> The reader must bear in mind that SIEVERS' classification of the "*Geräuschlaute*" (i. e. all except the vowels, liquids and nasals) is as follows:



distinction. The same is true of different dialects of the Armenian, while in those of Middle and South Germany, on the contrary, the surd *mediae* have for a long time held exclusive sway."

BRUGMAN's arguments (*Kuhn's Zeitschrift*, 24, page 6) are quite similar, except that he would allow only a short duration to such transition-periods, whereas SIEVERS assumes that they may occupy a long time.

It is evident that it would be vain to dispute about such very elastic terms as "long" and "short". It is of far greater importance to collect further facts from living languages, in order to draw conclusions from them with regard to the ancient languages. In the Greek field we might bring up the unstable rhotacism of the old Elian inscriptions, where, as is well known, τοῖς and τοῖρ etc. stand side by side. How great the difference of pronunciation really was between σ and ρ, unfortunately cannot be established; perhaps at the end of a word instead of σ a sound like the Sanskrit *visarga* was spoken, and possibly the terminal ρ was not dental, but guttural, in which case the actual difference between the two sounds must have been very trifling. It is quite impossible to assume that two phonetic forms, between which the speaking individual vibrates, can differ to any considerable extent in an Indo-European language, while in other linguistic fields, for example, in the languages of the American aborigines, this difference may be quite a wide one.<sup>1)</sup>

Meanwhile, whatever may be the judgment in regard to these single points, all agree in the theoretical recognition of the possibility of transition-periods. On the contrary, opinions differ as to the propriety of assuming within the domain of phonetic change a *permanent* lack of uniformity in the treat-

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1. Explosive sounds.
    - a. Surds (*tenues* and surd *mediae*).
    - b. Sonants (sonant *mediae*).
  2. Spirants.
    - a. Surds.
    - b. Sonants. [Transl.]

<sup>1)</sup> It is impossible, however, to pronounce with certainty upon the dialect of Elis, because inaccuracies and arbitrary changes may have taken place in transferring the sound to the written character.

ment of one and the same sound. The chief questions to be discussed in this connection are the following :

*First:* Can it be assumed that a phonetic change appears in one series of words, and not in others?

*Secondly:* Can it be assumed that one and the same word-form may, by a phonetic process, develop into permanently different forms?

The first question was formerly answered with an unquestioning affirmative whenever it was practically applied; thus BOPP found no difficulty in assuming that although the *s* of the aorist was in Greek regularly represented by *σ*, yet by an exception it appeared as *x* in ἦχα, ἔθηχα, ἔδωχα; nor did he trouble himself to discover any special reason for this remarkable exception. In proportion as phonetic change received more critical attention, such assumptions were naturally regarded with more and more suspicion, and they are resolutely rejected on principle by a number of philologists. The subject has recently been theoretically treated by BRUGMAN, *Kuhn's Zeitschrift*, 24, page 4, and BEZZENBERGER, *Gött. gel. Anzeigen*, May 21, 1879.

The possibility of a dissimilar treatment of sounds in different words might be explained from two stand-points. In the first place, the assumption might be made that every phonetic change begins with a definite word, and is propagated farther from this starting-point, so that it proceeds, for example, from one substantive to others, from these to adjectives and participles, and thence to the verb. Under this supposition it would be easy to imagine that certain words should not be affected by a phonetic change; that, for instance, in the case of prepositions, adverbs and other comparatively isolated words, the ancient phonetic condition should be retained, while in other words a new one should prevail. But evidently such an assumption would not correspond to the facts, and BRUGMAN seems to me to be right in describing the process as follows:

"If at a definite period a number of individuals change an *r*, which they have hitherto regularly pronounced as dental, into a guttural *r*, or if they give a deeper coloring to their pronunciation of *a* before *l*, which they had previously uttered

with a lighter *timbre*, this change does not begin with single definite words, so that what was at first only applicable to these single words is gradually transferred to others, — but the change begins *with the organs of speech themselves*, and we must expect that *every r* hitherto spoken as a dental, in whatever word and whatever category of words it stands, and similarly *every a* before *l*, which was hitherto spoken with a lighter *timbre*, will experience the change when it passes through the vocal organs."

Only the expression "the change begins with the organs of speech themselves" is not a happy one, since it can be understood to mean that every phonetic change begins with a physical change in the larynx etc. I would therefore rather say: the change begins with the pronunciation of the sounds.

It is not only shown by experience with popular dialects that this is actually the case, but we must consider that it is only under the supposition of a uniform and consistent pronunciation of sounds that the acquisition of a foreign language is explicable.

Another way of explaining the matter is intimated by BEZZENBERGER, page 652 of the article quoted above. He intrenches himself on the unassailable ground that different phonetic tendencies can arise at two points of the same linguistic territory; thus in one part a certain *k*-sound can become *s*, in the other *sz* (i. e. *sh*). Now in his opinion, an adjustment takes place between the different tendencies, and the result is that in certain words *sz* appears, in others *s*. Thus in Lithuanian *sz* has become the regular representative of one of the Indo-European *k*-sounds, but in *visas* and *sauja* *s* has become established in its stead. I cannot accept this view. It would recommend itself more strongly than it does (although even then it would not be the only possible explanation) if it were an actual fact that the two different sounds, so to speak, share the word-material of the language between them. But this is not a fact, for (to speak in BEZZENBERGER's language) one tendency has almost wholly outweighed the other, of which only scanty traces remain. How extraordinary that the speakers, who in a hundred cases employ *sz*, in one or two condescended to adopt *s*! — and why did this happen

in precisely these cases? Is it not much more natural to assume that the isolated exception to the empirical rule owes its origin not at all to phonetic change as such, but to some other cause, even although (as in the present instance) we cannot succeed in determining what this special cause is? BEZZENBERGER in the above article did give a somewhat different turn to the same supposition, by assuming that two phonetic tendencies, starting from different points and then coming together, might so adjust themselves that one phonetic form should appear in one category of forms, the other in another. Thus (if I understand him aright) when in Middle Frankish the *tenuis* *t* appears throughout changed by permutation [*verschoben*] to *z*, with the exception of the neuter *t* in *dat*, *wat*, *it*, *allet* and *dit*, he explains the process as follows: the tendency came from one direction to retain the *t*, from another to change it, and the condition in which the Middle Frankish is transmitted to us represents a compromise between the two tendencies. But PAUL has shown, in PAUL & BRAUNE's *Beiträge*, 6, page 554, that this case must be explained in a different manner. The retained *t*'s of the Middle Frankish stand at the end of the syllable, and it is probable that the terminal *t*'s in general were not changed according to phonetic laws, so that an original inflection like *fat*, *fazzes* must be assumed. Now *fat* was attracted by *fazzes*, and became *faz*, but where there were no such attracting oblique cases the *t* remained, i. e. in *dat*, *wat*, *it*, *dit* and *allet*.

But even if the explanation were doubtful in single instances, I should still be inclined to refuse acceptance to BEZZENBERGER's view, from the general ground alone that it presupposes a too great exercise of the reflective powers on the part of the speaker. I am therefore of the opinion that another explanation must likewise be sought for the few analogous cases which BEZZENBERGER adduces in addition to the above.

It accordingly seems to me that the first of our questions must be answered in the negative.

I am of the same opinion with regard to the second question, viz.: can it be assumed that one and the same word-form may, by a phonetic process, develop into permanently different forms? I find myself compelled, with LESKIEN, OSTHOFF,

BRUGMAN and others to answer this question also in the negative, although two scholars so eminent and so often differing in opinion as BANSKY and CURTIUS answer it in the affirmative.

A classical example for this assumption are the twin-forms  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$ ,  $\pi\epsilon\iota\lambda\omicron\omicron\varsigma$ , which are supposed to have arisen from the common primitive form  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$ .  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$  became (according to CURTIUS) on the one hand  $\pi\epsilon\iota\lambda\omicron\omicron\varsigma$  ( $\alpha$  = nasal vowel), then  $\pi\epsilon\iota\lambda\omicron\omicron\omicron\varsigma$ ,  $\pi\epsilon\iota\lambda\omicron\omicron\varsigma$ , and finally  $\pi\epsilon\iota\lambda\omicron\omicron\varsigma$ ; on the other hand it became  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$ , then  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$ . In order to form a clear idea of the process of development, we might assume that this double treatment originated at one point of the linguistic domain. Then it would be necessary to imagine that the single individual tried first one, then the other change of the group  $\nu\varsigma$ , and retained both in his memory. Now it seems to me impossible to find any reason why a speaking individual should waver in this fashion, and so obstinately continue to waver: and it is still more difficult to comprehend what could induce the rest to follow his example, when all the time the meaning of the two forms was identical, so that it could not be of the slightest practical interest to distinguish them. I cannot, accordingly, believe that processes like the above-mentioned actually take place in language. The other possible assumption seems to me quite as little justifiable, viz., that the impulse to separate the forms arose from two different points in the linguistic domain. It might, indeed, be assumed that  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$  perhaps originated in the West, and  $\pi\epsilon\iota\lambda\omicron\omicron\varsigma$  in the East, and that then an interchange took place between the two halves of the language. That the further development of a phonetic group should be different at the different points of a linguistic domain is naturally not beyond the range of possibility: but that the germs of difference could unfold as must be assumed in the case of  $\pi\epsilon\iota\lambda\omicron\omicron\varsigma$  by the side of  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$ , would only be possible if the adjusting process were interrupted, that is, if the intercourse were no longer unbroken. Under this supposition, it would be easy to understand how two dialects might be formed, one of which should have the form  $\pi\epsilon\iota\lambda\omicron\omicron\varsigma$ , the other the form  $\pi\epsilon\iota\lambda\omicron\nu\omicron\varsigma$ ; but why these two dialects should borrow each other's form is left unexplained.

From whichever side I view the matter, I am unable to comprehend how  $\mu\epsilon\iota\zeta\omicron\upsilon\varsigma$  and  $\mu\epsilon\iota\zeta\omicron\nu\omicron\varsigma$  could have both been formed in the same dialect from  $\mu\epsilon\iota\zeta\omicron\nu\omicron\varsigma$ , and am accordingly of the opinion that only the form  $\mu\epsilon\iota\zeta\omicron\upsilon\varsigma$  arose by a phonetic process from the primitive form  $\mu\epsilon\iota\zeta\omicron\omicron\omicron\varsigma$  (Sanskrit *māhiyasas*), while  $\mu\epsilon\iota\zeta\omicron\nu\omicron\varsigma$  is a formation by analogy from the nominative  $\mu\epsilon\iota\zeta\omicron\nu$ . How the *n* can be explained in the latter is, indeed, a point where there is room for disagreement. (V. the detailed discussion of BRUGMAN in *Kuhn's Zeitschrift*, 24, page 1 seq.)

We are now prepared to give a comprehensive answer to the question proposed in the beginning, viz., do phonetic laws as such admit of no exceptions?

We have seen where we may expect to meet with such laws. Certainly not in the collective mass of any existing speech, whether it be a popular dialect or a literary language. For it is not probable that all the individuals within a linguistic community will speak precisely alike. Therefore we can only expect to find these laws in the case of the single individual, or rather, if we wish to be quite exact, only in the average speech of an individual at any one moment. Now from what an individual speaks or would speak at a definite moment of his life, if he allowed the whole mass of his vocabulary to pass through his vocal organs, we must first subtract all that can be regarded as borrowed (in the broadest sense), and then all phonetic formations which depend upon the action of analogy. When this is done, the form which remains is the result of phonetic change alone. Here, and only here — leaving out of account the possible fluctuations of a transitional stage — we may expect complete uniformity in the treatment of all analogous cases, and in this sense we must assert that phonetic laws as such admit of no exceptions.

At the same time, it must be confessed that complete uniformity of phonetic change exists nowhere in the world of actual fact; but there are sufficient grounds for assuming that regularly occurring phonetic change is one of the factors to whose united action the empirical form of language is due. In single instances, it is true, it will only be possible to approximately reproduce this factor in its purity.

We can see at once from the above discussion whether and how far we are able to speak of "laws", or still more, of "natural laws", within the field of phonetics.

It has been shown that the phonetic laws which we postulate are nothing but uniformities which appear in a certain language and period, for which alone they are valid. Whether the expression "law" is really applicable here is doubtful. Yet I avoid entering upon a discussion of the notion "law", as employed in natural science and statistics, because I find that the term "phonetic law" has become so fixed by usage that it cannot be eradicated, and furthermore, because I can propose no better expression in its stead. It is also a harmless term, if we keep in mind that it can have no other sense than that defined above.

I cannot approve of characterizing phonetic laws as "natural laws". These historical uniformities can evidently bear no resemblance to chemical or physical laws. Language is a result of human action, and consequently phonetic laws are not based upon the regularity of natural processes, but upon that of apparently arbitrary human activities.

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## CHAPTER VII.

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### THE SEPARATION OF THE RACES.

As we mentioned on page 1, Sir WILLIAM JONES, as early as the year 1786, remarked that every philologist who compares Sanskrit, Greek and Latin necessarily arrives at the conclusion that these three languages must be derived from a common source, which perhaps no longer exists, while there are no such decisive grounds for assuming the same relation for Gothic and Celtic. We found that FRIEDRICH SCHLEGEL took a backward step in comparison with JONES, since he came to the conclusion that the Sanskrit language is the older, the

others younger and derived from it. Even BOPP, in the beginning of his literary career, does not always express himself correctly; thus in his *Conjugationssystem*, page 9, he speaks of the languages which "spring from the Sanskrit, or with it from a common mother"; but later he rightly characterizes the relation as a sisterly one. He is also on his guard not to overestimate the originality and antiquity of Sanskrit. Thus in the first edition of the Comparative Grammar there is a note (subsequently omitted) to § 605, which runs as follows :

"In my *Conjugationssystem*, and in the *Annals of Oriental Literature* (London, 1820), I have called attention to the fact that the Sanskrit second person plural *tutupá* is a mutilated form, and in the earlier sections of this book allusion has often been made to the fact that in single instances the Sanskrit is at a disadvantage compared with its European sister-idioms. It therefore surprised me that Prof. HÖFER in his work *Beiträge* etc., page 40, made the sweeping assertion that the new investigators have not succeeded in 'wholly emancipating themselves from the unhappy delusion that the Sanskrit has preserved its original perfection of structure with inviolable fidelity'. I for my part never ascribed to the Sanskrit such fidelity to its original structure, and it has always been a pleasure to me to call attention to the cases in which it must yield the palm to its European sisters" etc.

BOPP has no fixed name for the one ancestral speech [*Stammsprache*] from which the individual languages were derived. He speaks of the *one* ancestral speech, of the period of linguistic unity, of the primitive period of language, of the primeval formative period, etc. This one ancestral speech, which no longer exists, was in BOPP's opinion essentially similar to its sister languages. It is especially worthy of mention that he did not claim that it was incapable of change. Instead he assumes "that at the time of the identity of those languages which are now separated, many disturbances had already taken place in the organism of that one ancestral speech". (§ 673.) Thus he assumes that in oldest times the feminine in *ā* had an *-s* in the nominative, but had already lost it in the period of linguistic unity. I cannot find that BOPP expressed any conjecture regarding the home of the race which spoke



this primitive language, and he has in general no inclination to view things from an ethnological stand-point. The ethnological point of view was first emphasized by KUHN in the *Osterprogramm des Berliner Realgymnasiums* for 1845. (Cf. WEBER, *Indische Studien*, 1, page 323.)

According to BOPP, the individual languages have freed themselves from the "primitive home" by an "individualizing" process. The expression "separation of languages" [*Sprachtrennung*] also occurs (§ 493). Of the nearer or more remote relationship, i. e. of the order followed in the separation of the languages, BOPP's opinion was as follows: in Asia the Sanskrit and Medo-Persian are intimately connected; in Europe the Greek and Latin. In regard to the position of the Slavonic BOPP's opinion changed in the course of time. First (*Vergl. Gram.*, 1<sup>st</sup> edition, page 760) he considered the Lithuanian, Slavonic and German as "triplets"; later (*Ueber die Sprache der alten Preussen, Abh. der Berl. Akad.*, 1853, page 80) he defined his view thus: "The separation of the Slavo-Lithuanian idioms from the Asiatic sister-language, whether we call this Sanskrit or leave it without a name, is of later date than that of the classic, Germanic and Celtic languages, yet prior to the bifurcation of the Asiatic portion of our linguistic domain into the Medo-Persian and Indian branches." He did not assume a special relationship between the languages of the Celts and Romans.

SCHLEICHER was the first to establish a formal system of ramification for the Indo-European languages (under the figure of a genealogical tree). He agreed with BOPP in his assumption of a closer relationship between the Indian and Iranian branches (which is, indeed, irrefutable), and between the Italic and Greek languages, but differed from him in regard to the position of the Slavo-Lithuanian. He attempted to prove that the similarity of phonetic structure, which indubitably exists between the Asiatic languages and the Slavo-Lithuanian, does not date from primitive times, but originated in each group individually. Thus he assumes that the word for "hundred" in the parent speech was *kantam*, and that from this, after the separation of the primitive race into two, *çatam*

was developed in the Asiatic division, and *sŭto* in the Slavonic, quite independently of each other; so that the similarity between *ç* and *s* in this word, in which the Greek and Latin have preserved the old *k*, could not furnish any basis for genealogical conclusions. (Cf. *Beiträge*, I, page 107.) Accordingly, he wholly separates the Slavo-Lithuanian from the Asiatic division, and with JACOB GRIMM places it with the Germanic group. The chief proof of the close relationship of these languages consists in their agreement in the dative plural, where they exhibit an *m*, while the other languages have *bh* (e. g. Slavonic *vlŭkomŭ* and Gothic *vulfam*, but Sanskrit *vŕkebhyaſ*). Further, since SCHLEICHER places the Celtic with the Italic (*Beiträge*, I, 437), he obtains the following three groups: 1) Asiatic; 2) Slavo-Germanic; 3) Greco-Italo-Celtic. He defined the historical relation between these groups according to the fidelity with which each (in his opinion) has retained the primitive type. This fidelity seemed to him least in the Slavo-Germanic branch; he therefore assumed that this division was first separated from the primitive race, and then the Greco-Italo-Celtic, so that the Asiatic group alone remained.

It is plain, however, that this chronological classification depends upon a very questionable line of argument. The more advanced phonetic decay of the Slavo-Germanic (if, indeed, it can be regarded as proved) may be simply owing to the fact that the Slavo-Germanic has developed more quickly than its sister-tongues. SCHLEICHER does not, therefore, adduce sufficient grounds for dividing the Slavo-Germanic from the great European mass to which it geographically belongs. That it also belongs there from linguistic considerations was shown by LOTTNER in *Kuhn's Zeitschrift*, 7, page 18 seq. He establishes two great groups, the Asiatic and the European, the latter being especially characterized by a common *l* in opposition to the Asiatic *r* (e. g. *πολύ*, Gothic *filu*, as opposed to Sanskrit *purŭ*). A further characteristic was added by G. CURTIUS, in the *e* which appears uniformly in many positions, in opposition to the Asiatic *a* (e. g. *φέρω*, *fero*, Gothic *baira*, i. e. *bëra*, as opposed to *bhārāmi*). Thus the supposition seemed very probable that the Indo-Europeans, who spoke a uniform language while they were together, first split apart into Euro-

peans on the one hand and Asiatics on the other, and that after the separation certain peculiarities were developed in both groups, as, for example, the European *e*, which subsequently clung to all the subdivisions of the main group. For the European branch it seemed necessary to make two such subdivisions, the northern and southern, of which the former was again divided into Slavonic and Germanic, the latter into Greek, Italic and Celtic.

The Greek was here the hardest to dispose of. Some scholars assumed that the Celtic first freed itself from the South-European mass, after which the Greek and Italic remained together for a while; others (like SCHLEICHER) advocated the closer community of the Italic and Celtic; others, finally, divided the Greek wholly from Europe, and transferred it to Asia. This is the decision of GRASSMANN (*Kuhn's Zeitschrift*, 12, page 119), who speaks with great certainty of the many phenomena "in which the far-reaching harmony between a Greek and Aryan (pre-Brahmanic) nature becomes evident to us in language, poesy, mythology and life, and bears witness to the powerful intellectual development which the ancestral Greco-Aryan race passed through after its separation from the other branches." SONNE expresses the same opinion in his apparently forgotten article<sup>1)</sup>: *Zur ethnographischen Stellung der Griechen*, Wismar, 1869.

All these hypotheses, so far as they involve the idea of the separation of races or languages, were opposed by JOHANNES SCHMIDT, in an essay on the relationship of the Indo-European languages (*Die Verwandtschaftsverhältnisse der indogermanischen Sprachen*, Weimar, 1872). JOHANNES SCHMIDT starts from the same point where SCHLEICHER's opposition to BOPP began, namely, the relation of the Slavo-Lithuanian to the Asiatic, but considers BOPP essentially in the right. It is, indeed, very remarkable that in both groups the *k* of *kantam* becomes a sibilant (or something similar), while the *k* of *ka*

<sup>1)</sup> I take the occasion to quote a sentence from this article:

"But if in Sanskrit the verb of the main sentence assumes an unaccented form relatively to every preceding objective determination, I think we must recognize in this phenomenon, which is so thoroughly opposed to our European ideas, a remnant of pro-ethnical accentuation." (Page 3.)

"who" remains. Should not this remarkable agreement be explained as a result of common development, and is not SCHLEICHER's assumption of historical accident inadmissible? If, however, BORR's view is correct, there is no break between Asia and Europe, but only a "continuous transition" [*kontinuierliche Vermittelung*]. And SCHMIDT finds the same state of affairs in Europe. He recognizes that Greek, Italic and Celtic are intimately connected; but they do not form a historically distinct group, for as the Italic occupies an intermediate position between Greek and Celtic, the Celtic, on the other hand, is intermediate between Italic and Germanic, and further, the Germanic between Celtic and Slavonic, etc. Thus we can compare the Indo-European languages to a great chain of different rings, so linked together that it has neither beginning nor end. If we begin arbitrarily with the Indo-Iranian, the next ring will be the Slavo-Lithuanian, then the Germanic, Celtic, Italic, until the Greek is finally interlinked with the Indo-Iranian. The Armenian, which has been more accurately investigated only within the last few years, would take its place between the Indo-Iranian and Greek.

It will readily be seen that this transition or "wave-theory" (as its originator christens it, since the progressive movement within the bounds of language can be compared with the motion of the waves) agrees with the ramification theory in giving weight to the points of agreement (some of which have been mentioned) between the separate Indo-European languages, but differs from it in assuming a continuous transition in place of ramification. We accordingly must first examine this assumption. I am of opinion that the transition theory is untenable, if it is understood in the sense that a continuous transition takes place between all Indo-European languages, as they are historically transmitted to us. Against it we have the fact that the separate languages form independent unities, each shut off from the others. It is true that we may be in doubt under which group single dialects (e. g. within the Germanic family) are to be ranged; but with the chief languages, as for instance the Germanic in its relation to the Slavonic, the case is different. There could never be a doubt whether a certain linguistic mass were Slavonic or Germanic;

fixed boundaries exist between Germanic and Slavonic, as well as between the other chief languages. We are accordingly led to suppose that formerly, when the Germanic was spoken by fewer people, it constituted an uninterrupted field of intercourse, within which the separate Germanic dialects were developed in the course of time. The same is true of the other languages. And even if we were willing to make the assumption (which it seems to me cannot be proved, in spite of the ingenuity expended upon it) that the neighboring domains of two adjacent languages, like the Slavonic and Germanic, stand in closer relation than those more remote from each other, this would only prove that single peculiarities of the former boundary-region had passed over into the two divided territories, and that the position of the parts of each domain had suffered no great displacement; the assumption would still remain possible that the separate Indo-European languages have been divided from each other for a long period by boundaries preventing intercourse. The transition hypothesis must therefore be understood in the sense that in primitive times the languages did indeed form one connected whole, in the manner described by SCHMIDT, but that then boundaries preventing intercourse were formed, and thus a separate life began, which subsequently gained a rich historical development. This modification of SCHMIDT's hypothesis, which evidently recommends itself by its universal historical probability, is due to LESKIEN (*Die Declination im Slawisch-Litauischen und Germanischen*, Leipzig, 1876). It would accordingly seem that the transition and ramification hypotheses do not unconditionally exclude each other, but are to a certain extent compatible.

Unfortunately an objection must be noticed, which proceeds from the stand-point of more recent investigations, and is opposed to both the ramification and the transition hypothesis. That is, it has been discovered, by the investigations of the last few years, that the data from which it was customary to draw conclusions in regard to the closer relationship of individual languages are not so decisive as was hitherto assumed.

In general, it is clear that not every point of identity between two languages can be regarded as an argument for an

original community of life. If, for example, some languages have lost the augment, which is still possessed by others, of course it does not follow that this loss necessarily took place during the common life of these languages. It must also be admitted that identity of vocabulary (unless this appears to an overwhelming extent) cannot be used to prove an original community of life, because the possibility always remains that a word which we only find in certain languages existed also in the others, although it has been effaced by the ravages of time. Our material is sensibly diminished by these considerations, so that, strictly speaking, we have as conclusive evidence only those *new formations which are developed in common*. Under this head were ranked until recently the division of the unitary Indo-European *k* into *k* and *s* (*sz*) in both the Asiatic and Slavo-Lithuanian families; the *e* of the European languages; the *r* in the middle and passive of Italic and Celtic; and the *m* in the Slavo-Lithuanian and Germanic dative plural. But another explanation for these facts has very recently presented itself. It is often assumed (as remarked above) that these cases are not examples of new formations in the individual languages, but that the manifoldness must be traced back to the primitive speech. FICK took the lead with his paper on the linguistic unity of the Indo-Europeans of Europe (*Die Spracheinheit der Indogermanen Europa's*, Göttingen, 1873), in which, following ASCOLI, he showed that the two sounds of the Asiatic and Slavo-Lithuanian which were previously supposed to have originated from *k* were really the regular representatives of two different Indo-European *k*'s (v. above, page 52). Then followed the very probable theory (also referred to above) that *e* belonged to the primitive speech; further, that the *r* of the middle and passive in Italic and Celtic may possibly stand in connection with the *r* of the Indian *-re*, *-rate* etc. (cf. WINDISCH, *Beiträge von Kuhn und Schleicher*, 8, page 465, note); and that the *m* of Slavonic and Germanic perhaps belonged originally not to the *bh*-suffix, but to another.

If, now, this whole mode of reasoning is justified (as I assume), then from such differences as these, which reach back into the primitive speech, no conclusions can be drawn

## INDEX.

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- Abstract verb, v. Substantive verb.  
 Adaptation theory of A. LUDWIG, 66 seq.  
 ADRLUNG, 9, 80.  
 Agglutination theory: a name for Bopp's explanation of inflectional forms, 16; agglutination in the individual languages, 14, 46, 57; judgment of this theory, 61 seq.  
 AHRENS, mentions GRIMM with gratitude, 33.  
 Analogy and analogical formations, more frequently assumed in recent times, 58, 60; nature and varieties of these formations, 107 seq.; material and formal analogy, 111.  
 Aorist stem, its *s*, 6, 14, 95.  
 Armenian, its vocalism, 59; position, 135.  
 Aryan, use of the name, 2, note.  
 ASCOLI: his estimate of CORSEN, 40; gutturals, 59; his theory of roots, 83; of the aorist stem, 96; the suffix *anti*, 97; his Indo-European-Semitic parent speech, 100; phonetics, 120.  
 AUFRECHT, 37.  
 Augment, 14, 31.  
 BECKER, C. F., 62.  
 BRUGMANN, 84.  
 BENFAY, 35; his estimate of Bopp, 3; of CORSEN, 40; his primary verbs, 78; theory of suffixes, 87 seq.; the present stem in *nu*, 93; phonetic change, 119; individualizing and equalizing forces in language, 120, 128.  
 BERGAIGNE, 92.  
 BERNHARDI, A. F., his theory of the parts of speech, 7.  
 BRZENBERGER: roots, 90; phonetic laws, 115, note, 127; on the *sig*-aorist, 95, note.  
 BOPP, 1 seq.; his view of the originality of the vowel *a*, 51; of the separation of the languages, 131.  
 BÖTTLINGK, Yakut Grammar, 71.  
 BÖTTLINGK and ROTH, Sanskritlexicon, 38.  
 Borrowed words, 113 seq., 116 note.  
 BRÉAL, 17, 106.  
 BRUGMAN: his views on vocalism, "nasalis sonans", 59, 60; phonetic laws, 61, note, 124, 125; the *sig*-aorist, 95; *anti* a noun-suffix, 97; the imperative suffix *tāt*, 98, note; the personal endings of the second person, 99; formations by analogy, 108; the *ε* of the sigmatic aorist, 112, note.  
 BUTTMANN, 34.  
 CAREY, 10.  
 Causatives, their sign according to Bopp, 14.  
 Celtic, its position, 1, 38, 132 seq.  
 Climate, its influence on phonetic action, 117 seq.  
 COLLITZ, 59.  
 Consonants, v. Phonetics.  
 "Contamination", 111.  
 CORSEN, 34, 40, 55.  
 CUETIUS: his *Grundsätze der griechischen Etymologie*, 39; position in regard to phonetic laws, 39, 101 seq., 115, 116, note, 119, 128; his chronologico-linguistic labors, 55, 100; the splitting of the vowel *a*, 58; roots, 77, 80, 81 (against Pott's theory of root-composition); stem-forming suffixes, 86, 87; borrowed

- words, 114; the separation of the languages, 133.
- Desideratives, their sign according to Bopp, 14.
- Determinatives, v. Root.
- Differentiation, impulse to, 109.
- "Doublets", 106, 128.
- Evolution theory, 62 seq.
- FICK, 47; Indo-European *e*, 59; his theory of roots, 81; stem-forming suffixes, 90, 94; the separation of the languages, 137.
- FORSTER, 32.
- Fulda, 10.
- Future, its sign, 6, 14.
- Genealogical tree for language, v. Ramification theory.
- Germanic family, its position according to JONES, 1; according to Bopp, 132.
- Greco-Italic unity, 138.
- Grammarians, Indian, 29, 37.
- GRASSMANN: prepositions, 79; the position of Greek, 134.
- Gravity, Bopp's law of, 20.
- GRIMM, J., 32 seq.; the originality of the vowel *a*, 51; roots, 78; relation of Slavonic and Germanic, 133.
- HÄGEL, his influence on SCHLEICHER, 40 seq.
- HERMANN, G., 8.
- HUMBOLDT, W. VON, 26 seq.
- Indo-European, the name, 2, note.
- Inflection, SCHLEICHER's idea of, 46; cf. Agglutination.
- JONES, 1, 2, 130.
- KUHN, A., 37, 47, 86, 93, 98, 132.
- KUHN, E., 115, note.
- LASSEN, 30, 62.
- Lautverschiebung*, law of, 33; in Middle Frankish, 127.
- Lenep, 4, note, 5.
- LASKIEN: phonetic laws, 60, note, 113, 127; the separation of languages, 136.
- LOTTNER, 133.
- LUDWIG, 66 seq., 92.
- MERGENT, 57.
- Method: of linguistic comparison, 26; of natural science, 43, 54.
- MEYER, G., 112, note.
- MEYER, L., 87.
- MICHAELIS, CAROLINA, 110.
- MIKLOSICH, 44.
- MISTELI, 102 seq.
- Mixed languages, 122, note.
- Modes, 96.
- MOMMSEN, 138, note.
- MÜLLER, M., 4, note, 37, 77.
- "Nasalis sonans", v. Phonetics.
- Neue, 109.
- Optative, 7, 12, 57, 96.
- Organism, organic, as used by Bopp, 18.
- OSTHOFF: the *r*-vowel in the parent speech, 60; phonetic laws, 61, note; formations by analogy, 111, note; influence of the climate on phonetic change, 117, seq., 127.
- Parent speech, Indo-European, 1, 48 seq., 52, 56, 100, 131; Indo-European-Semitic, 100.
- Parts of speech, 7, 77, note.
- PAUL, 111, note, 127.
- Permutation of consonants, v. *Lautverschiebung*.
- Phonetics, phonetic laws: in general: 21 seq., 35, 58 seq., 102 seq., particular points: *a*-vowels (*a*, *e*, *o*), 51 seq., 58 seq.; "nasalis sonans", 53, 60; two series of *k*'s, 53, 137; *r*-vowel in the parent speech, 60; vowel-strengthening, 84; two *f*'s, 117, note; influence of climate on phonetic change, 117 seq.; *r* in the passive endings of the Italic, 137.
- POTT, 23, 35 seq.; the suffix *anti*, 46; roots, 74; position of prepositions in language, 79; "symbolical" explanation, 101.
- Prepositions, 79.
- Present stems, 14, 92 seq.
- Primitive speech, v. Parent speech.
- Ramification theory, 132 seq.
- RAPP, 62.
- RASK, 33.



- Root: the notion "root", 9, 13, 73 seq.; Indian lists of roots, 10, 37; Porro's analysis of roots, 35, 81; classes of roots, 45, 77 seq.; roots not present in the individual languages, 76; form of roots, 79 seq.; root-determinatives, 81; dissyllabic roots, 82 seq.
- ROSEN, 37.
- ROTH, 37.
- SCHMID, 4, note, 5, 11.
- SCHREIER: analogy, 58; roots, 78; suffixes, 86 seq.; the subjunctive, 96; the imperative suffix *tāt*, 98, note; the chronology of language, 100.
- SCHLEGEL, FR. VON, 1, 3 seq., 130.
- SCHLEGEL, A. W. VON, 28 seq.
- SCHLEICHER, 19, 26, 40 seq.: the vocalism of roots, 80, 84; middle endings, 99; the separation of languages, 132.
- SCHMIDT, J.: the optative, 57, 96; reconstructions of the parentspeech, 57, 101; gutturals and the *e* of the parent speech, 59; his so-called "wave-theory", 134 seq.
- SIEVERS, 123.
- Significance of sounds, its influence on their preservation, according to CURTIUS, 105, 106.
- Slavonic, its position according to BOPP, 132.
- SONNE, his view of the position of the Greek, 134.
- Sounds, v. Phonetics.
- STERNTHAL, 63, 75.
- Subjunctive, its sign, 46, 96.
- Substantive verb, 6 seq., 11, 31, 95.
- Suffixes: BENFAY's theory of suffixes, 87 seq.; SCHREIER on suffixes, 86, 89; suffixes derived from predicative roots, 87; stem-forming suffixes, 11, 85 seq.; suffix *tar*, 86; case-endings, 13, 91; ending of the genitive, 92; personal endings, 6, 11, 13, 62, 97 seq.; *anti*, 97, 98; middle endings, 45, 99; Latin middle and passive endings, 6, 137.
- Syllabic nasal, v. Phonetics.
- "Symbolical" explanation, 15, 36, 101.
- Tense-stems, 92 seq.
- TOOKER, H., 4, note.
- Transition theory, 135.
- Veda, 36, 37.
- Vowels, v. Phonetics.
- WACKERNAGEL, 112.
- "Wave-theory", v. Transition theory.
- WEBER, 78.
- WESTERGAARD, 37, 38.
- WESTPHAL, 63 seq.
- WHITNEY, 2, note, 58, 117, 119.
- WILKINS, 10.
- WILSON, 37.
- WINDISCH, 87, 137.
- ZIMMER, 89, note.

